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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATUM.

Page 359, line 7, *for* unchangeably *read* unchangeable, *and insert a comma.*

PLATES.

I. and II. Illustrative of Dr. Barry's Paper on the Spiral Structure of
    Muscle.

III. Illustrative of Dr. Andrews's Paper on a new Aspirator.

IV. Illustrative of Dr. Tyndall's Paper on the Reduction of Temperature
    by Electricity.

V. Illustrative of Mr. Grove's Paper on the Electro-chemical Polarity of
    Gases.

I. *Remarks on* Lord Brougham's *" Experiments and Observations on the Properties of Light,"* &c. *inserted in the Phil. Trans.* 1850, Part I. *By the* Rev. BADEN POWELL, *M.A.*, *F.R.S.* &c., *Savilian Professor of Geometry, Oxford**.

THE publication of Lord Brougham's optical researches, in which a number of experimental facts connected with the phænomena now usually called " diffraction," are viewed according to a peculiar theory of certain new properties of light, and in some respects held to be irreconcileable with the principle of interference, seems to render desirable some examination into the actual bearing of the results on the theory of undulations, by which not only all the phænomena of diffraction, hitherto known, have been so perfectly explained, but which has also been applied so extensively to other large classes of facts, as to render it unphilosophical to resort to theories assumed on independent grounds to meet apparent exceptions in limited classes of phænomena.

These researches having been briefly alluded to by the Astronomer Royal in his opening address to the British Association at Ipswich†, and having also myself made a few observations on the subject at the same meeting‡, my object in the present communication is to follow up the question in somewhat more detail; and without pretending to enter on any controversy as to the author's *theory*, to examine merely the *experimental evidence* adduced, and inquire how far it seems accordant or not with the undulatory theory.

During the summer of last year I took the opportunity of

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* Communicated by the Author.   † See Athenæum, No. 1236.
‡ See Ibid. No. 1237.

repeating the experiments with the utmost care, for all the most material cases considered; since which time various causes have delayed the publication of my results.

The whole of the author's investigations are expressed with reference to his peculiar hypothesis of certain forces of "deflexion" and "inflexion" supposed to be exerted upon the rays of light by the action of the edge of an opake body near which they pass: nor is it always an easy matter to disentangle the actual facts from the language of this theory, so as to see to what the experimental evidence really amounts.

Of those of the author's propositions which refer solely to the exposition of his theoretical views, I do not propose to enter on any discussion. There are also other portions of the investigations, which, though of a more experimental character, will not call for much observation, as they either tend to establish phænomena in exact conformity with well-known results, or are of a nature not having much bearing on theory either way.

Of this class are the preliminary experiments (Prop. I. Exp. 1, 2, 3); though with respect to the last it ought to be remarked, that Newton by no means limits the number of fringes to *three*, and in one modification of the experiment expressly mentions that *four or five* were rendered visible*. When (as in Exp. 4) the origin of light is not the *single point absolutely requisite in all accurate investigations,* but an extended object, such as a flame, the moon, &c., it may be questioned how far the fringes may be properly termed images of it. In Prop. II. Exp. 2, that the nature or form of the edge makes no difference in the result, accords exactly with the long-known experiments of Biot, Haldat, and others. Indeed, as is equally well understood, the fringes may be produced without any opake edge at all, as at the junction of two faces cut on a glass, slightly inclined to each other. Again, the hyperbolic fringes of an acute angle (in Prop. V. Exp. 3), as well as the measures of the fringes at successive distances from the edge determining the locus of any given fringe (in Prop. X., and additional remarks, (2) p. 252), appear to agree with previous observations; though, according to the author's theory, each fringe seems to be regarded as an individual ray, while in the interference theory it is the locus of the intersections of a series of rays.

At another part of his discussion the author assumes (Prop. XI.) an aggressive position, and endeavours to *refute* the application of the interference theory. In reply I think it will suffice to remark, under the several heads,—(1) the theory of interference explains perfectly *both* the internal and external fringes of a shadow; (2) the *breadth* of the fringes has no dependence on

* See Opticks, book 3. part 2. obs. 2.

the *length of route* of the rays, but it has on the *angle* at which they intersect ; so that (3) in the case represented in the author's fig. (20), supposing abstractedly two pairs of interfering rays (such as BC, AC, and BD, AD), it is evident that the fringes at D *ought to be* broader than those at C, not owing to any differ-ence in the *routes*, but because the *angle* BDA is less than BCA ; while (4) interference perfectly explains fringes, even when the action is wholly on *one side* of the ray or edge.

But passing from these points of confessedly less importance, we will proceed to the most material and fundamental experiment (Prop. II. Exp. 1), in which, when fringes are formed by the edge of an opake body, if a second edge be placed at a greater distance along the ray from the origin on the same side as the first edge, it produces no change in the fringes, but on the opposite side it does, the fringes being shifted in position towards the first side ; or in other words, in the one case it has no power of producing further diffraction, in the other it has : and this is viewed by the author as supporting his theory of a peculiar action exerted by the edge upon the ray passing near it, by which it is disposed or indisposed for further flexure according to the conditions above expressed.

The experimental fact in general is easily verified. There is, however, one material condition necessary to be attended to for reproducing the result exactly as described by the author.

When two edges are at the same distance from the origin and from a narrow aperture, they give, as is well known, fringes on each side extending into the shadow, with a white centre (fig. 1). As one edge is removed successively further from the origin and nearer to the screen, the fringes on that side dilate (fig. 2),

Fig. 1. Fig. 2. Fig. 3.

become faint, and at length disappear (fig. 3); so that beyond a

certain distance there remain only the fringes on the other side, or on that of the edge nearest the origin, which diverge further into the shadow on that side as the breadth of the effective aperture is diminished.

In this way, then, the second edge, if beyond the limits of distance mentioned, will cause an appearance of fringes on the side towards the first edge diverging into the shadow.

With regard to the bearing of this experiment on theory, it is in the first instance necessary to bear in mind, that, *according to the undulatory theory, neither the formation of fringes, nor any shifting of those fringes, implies a* FLEXURE *in the rays; in this theory no such idea is introduced or needed.*

In the particular case in question, when the two edges are at the same distance from the origin forming a narrow aperture, the nature of the fringes is perfectly explained and reduced to quantitative results by Fresnel's theory.

When the second edge is placed as in Lord Brougham's experiments, at a greater distance along the ray, this would be equivalent to a wide aperture placed obliquely to the direction of the ray, so as to be effectively as narrow as before.   Now this case is one *which has not yet been reduced to calculation.*

The formulas of Fresnel, even in the simplest cases, are considerably complicated, and involve integrations which cannot be generally exhibited in a finite form.   In the cases of a single edge, or that of an aperture when it is a long narrow parallelogram, an equilateral triangle, or a circle, the integration has been performed in a way sufficient for calculation*.

In the case of the *oblique* aperture, at my request, a friend eminently versed in the analysis of the subject, undertook to work out the formulas; and he pursued the inquiry far enough to be able to say that they became immensely complicated; still it could not be certain that they might not be made to yield to proper treatment, should anyone think it worth while to follow up the attempt.

But further, this particular case *has been considered,* though only in a general way, by Fresnel†.   Upon the obvious geometrical construction he points out the general conditions for determining the position of a fringe, and shows that the fringes will in this case undergo a modification, and *will not be symmetrical,* but more expanded on one side than the other, which exactly agrees with observation.

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* See Airy's Tracts, Undulatory Theory, art. 73 *et seq.*   Journal of Science and Phil. Mag. vol. xv. Dec. 1839; and vol. xviii. Jan. 1841.

† *Mém. sur la Diffraction.   Mém. de l'Institut,* vol. v. note, p. 452, for 1821, published in 1826.

The simple facts affirmed in Prop. III. Exp. 1 and 2, when divested of all theoretical language, appear to be, that if three edges, E, F, G, be placed at successive distances from the origin in the order of the letters, E and G being on one side of the ray and F on the other; then if E and F alone give fringes as at *o* (fig. 4), and G be then made to act upon them, or if F and G alone give fringes and E be made to act upon them, in either case the fringes will be shifted to *p* towards the side on which E and G lie, and become broader; and the conclusion which the author chiefly insists upon is that all three edges act in producing the ultimate result: the same thing being further confirmed by exp. 3, in which a curved form given to the edge E, is still exhibited in the form of the fringes after the action of F and G.

Fig. 4.
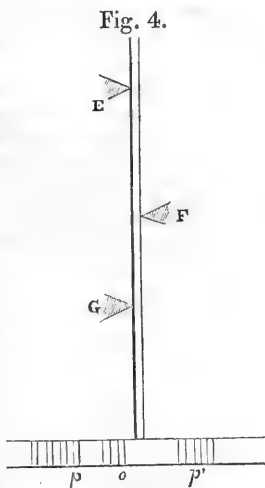
That all three edges should be in some degree effective in producing the ultimate character of the fringes, would, on a general view, be obviously consistent with the wave theory; since, on that theory, a new set of waves originates at each edge, all of which conspire to produce the ultimate result; though antecedent to exact calculation, it would be impossible to say what would be the precise action of each.

On repeating the experiment, however, in regard to the *particular appearances* described by the author, I have found considerable difficulty: consistently with the conditions before remarked, if the edges E and F form a narrow aperture so as to give a white centre, and within such limits of distance along the ray as to produce fringes on *each* side (as in fig. 2), then if G be also within the same limits, and be advanced so near to the ray laterally as to make a still narrower aperture, the fringes on *each side* will expand further into the shadow. If the edges be *beyond* those limits (which seems to be implied in the author's description, since he speaks of only *one* set of fringes), then E and F will give a white centre with fringes *on the side towards* E, as at *o*; and when G is introduced it will narrow the aperture and give new fringes *on the side towards* F, at *p'*, that is, just *the opposite way* to that which the author describes. In repeating the experiment a great number of times at very different distances, and under varied conditions, I have never been able to obtain any other re-

sult: indeed it would clearly be inconsistent with the former experiments that it should be otherwise.

Prop. VI. appears precisely to express Fresnel's conclusion (above referred to), that with two edges at unequal distances from the origin, the fringes will be broader on the side towards the edge most remote from the origin, which is again more precisely exhibited in Prop. VIII., when the aperture is sufficiently narrow to give a white centred image; the same regard being had to the limits in distance as before.

In Prop. VII. the meaning is by no means obvious; but it seems to amount experimentally to this,—that with one edge only, the fringe nearest that edge is the broadest; and that when a second edge acts opposite to it at some distance along the ray, but so as to give the fringes of an aperture, then among the fringes of each set, those towards the middle of the aperture are the broadest: the first being obviously the case of the external fringes; the second easily verified, and agreeing with the ordinary case of an aperture with edges at the same distance; while as to the application of the undulatory theory, we can only make the same remarks as before.

In the Additional Observations, (3), p. 254, the truth of the general assertion, that when fringes are formed by two edges, a third can affect them only when parallel and not when at right angles to them, is indeed obvious; but the precise conditions of the experiment mentioned are difficult to understand. It would seem to consist in first forming the fringes of a narrow aperture with a broad white centre in the ordinary way; and then in that white centre producing new fringes by a third edge nearer to the screen: these, however, the author affirms, will be formed only when the third edge is parallel to the aperture and not when at right angles to it; they are also described as brighter and narrower than the ordinary fringes. The author cautions us against confounding them with the ordinary external fringes, and proceeds to argue that they are of a different nature, for several reasons, but chiefly because (Exp. 1) they do not increase in breadth when the aperture is narrowed, and (Exp. 2) because their breadth increases as the distance of the third edge from the aperture is diminished, the third edge remaining at the same distance from the screen.

The last results (which I have fully verified) do not appear to me to evince any *peculiarity*: relatively to the third edge, the aperture may be regarded as a new origin of light, in which light the third edge gives its external fringes.

But with respect to the first part of the proposition, viz. that these fringes are only formed parallel to the aperture, on repeatedly trying the experiment, I have uniformly found them

formed equally, whether the edge be parallel or perpendicular to the aperture; though in the latter case they may for obvious reasons be less distinct and conspicuous.

It might indeed be fully admitted that the rays forming the white centre may be in some respects under different conditions from the ordinary rays, and that thus the fringes formed in them might possibly be different: I can only say that I have never been able to detect any such difference.

If, indeed, the author's meaning be that these fringes extended in any degree into the lateral fringes, it is obvious that they would be mutually affected in a way conformable to previous experiments.

One other remark of the author deserves especial attention*; that, but for what he considers the incapacity for further flexure in the same direction, induced in a ray after one inflexion, that ray might be continually bent round an opake body; and thus a luminous object might be seen, though the whole of the body intervened, or in other words, that we might *see round a corner.*

Now if such inflexion took place it would clearly be always accompanied by a considerable diffusion of the light, so that after a few successive inflexions it might be so much weakened as to become imperceptible.

It is however a remarkable fact, that such an apparent inflexion *does take place* to a very great extent, as I have pointed out in a paper " On Luminous Rings round Shadows " (Memoirs of the Royal Astronomical Society, vol. xvi. p. 306), and which (as I have there mentioned) I believe to be a modification of the same phænomenon, described rather obscurely by Newton† and more distinctly by Hooke‡, and apparently accordant with the theory of undulations (Ibid. p. 310).

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* Additional Observations, 4.
† Opticks, book 3. part 1. obs. 5.
‡ As this curious point seems to have been much overlooked, I shall perhaps be excused in annexing a brief notice of Dr. Hooke's experiment, from a fragment on Light, appended to the Essay on Comets and Gravity, in his posthumous works: London, 1705, p. 186.

Light being admitted into a dark room through a very small hole and received on a screen at some distance, on holding an opake body in the cone of light, besides a " zone or fascia of light much brighter than the rest of the surface," along and outside the edge of the shadow (which was probably the first diffraction fringe), he observed *a faint light extending from the edge into the shadow*; and when the opake body was held so as to cover nearly the whole of the luminous circle, " rays were seen *darting downwards perpendicular to the edge of the shadow, like the tail of a comet, striking downwards more than* 10 *times, probably* 100 *times their breadth, or very near to a quadrant,*" and *growing fainter at greater distances.* The " rays" were obviously occasioned by irregularities in the edge; the rays were per-

I have thus, I trust with perfect impartiality, gone through all the main experimental points of the author's investigations, and upon the whole I can perceive *nothing substantiated* which is positively *irreconcileable with the principle of interference,* while the new modifications of the phænomena here presented, so far as *general* considerations can be relied on, seem sufficiently conformable to the undulatory theory : but as to their more *exact,* or *quantitative* explanation, no definitive opinion can be pronounced, until certain analytical investigations of almost impracticable length and complexity, shall have been gone through, by which alone that theory can be brought into exact and satisfactory comparison with experiment.

II. *On the Dynamical Theory of Heat, with numerical results deduced from* Mr. Joule's *equivalent of a Thermal Unit, and* M. Regnault's *Observations on Steam. By* WILLIAM THOMSON, *M.A., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow*.*

Introductory Notice.

1. SIR HUMPHRY DAVY, by his experiment of melting two pieces of ice by rubbing them together, established the following proposition :—"The phænomena of repulsion are not dependent on a peculiar elastic fluid for their existence, or caloric does not exist." And he concludes that heat consists of a motion excited among the particles of bodies. "To distinguish this motion from others, and to signify the cause of our sensation of heat," and of the expansion or expansive pressure produced in matter by heat, "the name *repulsive* motion has been adopted†."

pendicular to the edge ; if circular, tending to the centre ; if angular, bisecting it ; if concave, spreading out, &c. A representation of the appearance is given in Plate ii. fig. 8 (p. 155). At p. 190, the Editor adds a memorandum found among Dr. Hooke's papers, stating, that on March 18, 167$\frac{4}{5}$, he "read a discourse" on several new properties of light ; which he sums up as follows :—

"That there is a deflexion of light differing both from reflexion and refraction, and seeming to depend on the unequal density of the constituent parts of the ray, whereby light is dispersed from the place of condensation and rarefied or gradually diverged into a quadrant ;" 2ndly, that this takes place " perpendicularly to the edge ;" and 3rdly, that " the parts deflected by the greatest angle are the faintest."

I have fully referred to and commented upon Newton's description of the same phænomenon, conveyed in terms so singularly coincident, in my paper before referred to.

* From the Transactions of the Royal Society of Edinburgh, vol. xx. part 2. Passages added in the present reprint are enclosed in brackets.

† From Davy's first work, entitled " An Essay on Heat, Light, and the Combinations of Light," published in 1799, in " Contributions to Physical

2. The dynamical theory of heat, thus established by Sir Humphry Davy, is extended to radiant heat by the discovery of phænomena, especially those of the polarization of radiant heat, which render it excessively probable that heat propagated through "vacant space," or through diathermanic substances, consists of waves of transverse vibrations in an all-pervading medium.

3. The recent discoveries made by Mayer and Joule[*], of the generation of heat through the friction of fluids in motion, and by the magneto-electric excitation of galvanic currents, would either of them be sufficient to demonstrate the immateriality of heat; and would so afford, if required, a perfect confirmation of Sir Humphry Davy's views.

4. Considering it as thus established, that heat is not a substance, but a dynamical form of mechanical effect, we perceive that there must be an equivalence between mechanical work and heat, as between cause and effect. The first published statement of this principle appears to be in Mayer's *Bemerkungen über die Kräfte der unbelebten Natur*[†], which contains some correct views regarding the mutual convertibility of heat and mechanical effect, along with a false analogy between the approach of a weight to the earth and a diminution of the volume of a continuous substance, on which an attempt is founded to find numerically the mechanical equivalent of a given quantity of heat. In a paper published about fourteen months later, " On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat[‡]," Mr. Joule of Manchester expresses very distinctly the consequences regarding the mutual convertibility of heat and mechanical effect which follow from the fact, that heat is not a substance but a state of motion; and investigates on unquestionable principles the " absolute numerical relations," according to which heat is connected with mechanical power; verifying experimentally, that whenever heat is generated from purely mechanical action, and no other effect produced, whether it be by means of the friction of fluids or by the magneto-electric excita-

and Medical Knowledge, principally from the West of England, collected by Thomas Beddoes, M.D.," and republished in Dr. Davy's edition of his brother's collected works, vol. ii. Lond. 1836.

[*] In May 1842, Mayer announced in the *Annalen* of Wöhler and Liebig, that he had raised the temperature of water from 12° to 13° Cent. by agitating it. In August 1843, Joule announced to the British Association, "That heat is evolved by the passage of water through narrow tubes;" and that he had " obtained one degree of heat per lb. of water from a mechanical force capable of raising 770 lbs. to the height of one foot;" and that heat is generated when work is spent in turning a magneto-electric machine, or an electro-magnetic engine. (See his paper " On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat."— Phil. Mag., vol. xxiii. 1843.)

[†] *Annalen* of Wöhler and Liebig, May 1842.

[‡] British Association, August 1843; and Phil. Mag., Sept. 1843.

tion of galvanic currents, the same quantity is generated by the same amount of work spent; and determining the actual amount of work, in foot-pounds, required to generate a unit of heat, which he calls " the mechanical equivalent of heat." Since the publication of that paper, Mr. Joule has made numerous series of experiments for determining with as much accuracy as possible the mechanical equivalent of heat so defined, and has given accounts of them in various communications to the British Association, to the Philosophical Magazine, to the Royal Society, and to the French Institute.

5. Important contributions to the dynamical theory of heat have recently been made by Rankine and Clausius; who, by mathematical reasoning analogous to Carnot's on the motive power of heat, but founded on an axiom contrary to his fundamental axiom, have arrived at some remarkable conclusions. The researches of these authors have been published in the Transactions of this Society, and in Poggendorff's *Annalen,* during the past year; and they are more particularly referred to below in connexion with corresponding parts of the investigations at present laid before the Royal Society.

[Various statements regarding animal heat, and the heat of combustion and chemical combination, are made in the writings of Liebig, (as, for instance, the statement quoted in the foot-note added to § 18 below,) which virtually imply the convertibility of heat into mechanical effect, and which are inconsistent with any other than the dynamical theory of heat.]

6. The object of the present paper is threefold :—

(1.) To show what modifications of the conclusions arrived at by Carnot, and by others who have followed his peculiar mode of reasoning regarding the motive power of heat, must be made when the hypothesis of the dynamical theory, contrary as it is to Carnot's fundamental hypothesis, is adopted.

(2.) To point out the significance in the dynamical theory, of the numerical results deduced from Regnault's observations on steam, and communicated about two years ago to the Society, with an account of Carnot's theory, by the author of the present paper; and to show that by taking these numbers (subject to correction when accurate experimental data regarding the density of saturated steam shall have been afforded), in connexion with Joule's mechanical equivalent of a thermal unit, a complete theory of the motive power of heat, within the temperature limits of the experimental data, is obtained.

(3.) To point out some remarkable relations connecting the physical properties of all substances, established by reasoning analogous to that of Carnot, but founded in part on the contrary principle of the dynamical theory.

PART I.—*Fundamental Principles in the Theory of the Motive Power of Heat.*

7. According to an obvious principle, first introduced, however, into the theory of the motive power of heat by Carnot, mechanical effect produced in any process cannot be said to have been derived from a purely thermal source, unless at the end of the process all the materials used are in precisely the same physical and mechanical circumstances as they were at the beginning. In some conceivable "thermo-dynamic engines," as for instance Faraday's floating magnet, or Barlow's "wheel and axle," made to rotate and perform work uniformly by means of a current continuously excited by heat communicated to two metals in contact, or the thermo-electric rotatory apparatus devised by Marsh, which has been actually constructed; this condition is fulfilled at every instant. On the other hand, in all thermo-dynamic engines, founded on electrical agency, in which discontinuous galvanic currents, or pieces of soft iron in a variable state of magnetization are used, and in all engines founded on the alternate expansions and contractions of media, there are really alterations in the condition of materials; but, in accordance with the principle stated above, these alterations must be strictly periodical. In any such engine, the series of motions performed during a period, at the end of which the materials are restored to precisely the same condition as that in which they existed at the beginning, constitutes what will be called a complete cycle of its operations. Whenever in what follows, *the work done* or *the mechanical effect produced* by a thermo-dynamic engine is mentioned without qualification, it must be understood that the mechanical effect produced, either in a non-varying engine, or in a complete cycle, or any number of complete cycles of a periodical engine is meant.

8. The *source of heat* will always be supposed to be a hot body at a given constant temperature, put in contact with some part of the engine; and when any part of the engine is to be kept from rising in temperature (which can only be done by drawing off whatever heat is deposited in it), this will be supposed to be done by putting a cold body, which will be called the refrigerator, at a given constant temperature in contact with it.

9. The whole theory of the motive power of heat is founded on the two following propositions, due respectively to Joule, and to Carnot and Clausius.

Prop. I. (Joule).—When equal quantities of mechanical effect are produced by any means whatever from purely thermal sources, or lost in purely thermal effects, equal quantities of heat are put out of existence or are generated.

Prop. II. (Carnot and Clausius).—If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed, it produces as much mechanical effect as can be produced by any thermodynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat.

10. The former proposition is shown to be included in the general " principle of mechanical effect," and is so established beyond all doubt by the following demonstration.

11. By whatever direct effect the heat gained or lost by a body in any conceivable circumstances is tested, the measurement of its quantity may always be founded on a determination of the quantity of some standard substance, which it or any equal quantity of heat could raise from one standard temperature to another ; the test of equality between two quantities of heat being their capability of raising equal quantities of any substance from any temperature to the same higher temperature. Now, according to the dynamical theory of heat, the temperature of a substance can only be raised by working upon it in some way so as to produce increased thermal motions within it, besides effecting any modifications in the mutual distances or arrangements of its particles which may accompany a change of temperature. The work necessary to produce this total mechanical effect is of course proportional to the quantity of the substance raised from one standard temperature to another ; and therefore when a body, or a group of bodies, or a machine, parts with or receives heat, there is in reality mechanical effect produced from it, or taken into it, to an extent precisely proportional to the quantity of heat which it emits or absorbs. But the work which any external forces do upon it, the work done by its own molecular forces, and the amount by which the half *vis viva* of the thermal motions of all its parts is diminished, must together be equal to the mechanical effect produced from it ; and consequently, to the mechanical equivalent of the heat which it emits (which will be positive or negative, according as the sum of those terms is positive or negative). Now let there be either no molecular change or alteration of temperature in any part of the body, or, by a cycle of operations, let the temperature and physical condition be restored exactly to what they were at the beginning ; the second and third of the three parts of the work which it has to produce vanish ; and we conclude that the heat which it emits or absorbs will be the thermal equivalent of the work done upon it by external forces, or done by it against external forces ; which is the proposition to be proved.

12. The demonstration of the second proposition is founded on the following axiom :—

It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects[*].

13. To demonstrate the second proposition, let A and B be two thermo-dynamic engines, of which B satisfies the conditions expressed in the enunciation; and let, if possible, A derive more work from a given quantity of heat than B, when their sources and refrigerators are at the same temperatures, respectively. Then on account of the condition of complete *reversibility* in all its operations which it fulfils, B may be worked backwards, and made to restore any quantity of heat to its source, by the expenditure of the amount of work which, by its forward action, it would derive from the same quantity of heat. If, therefore, B be worked backwards, and made to restore to the source of A (which we may suppose to be adjustable to the engine B) as much heat as has been drawn from it during a certain period of the working of A, a smaller amount of work will be spent thus than was gained by the working of A. Hence, if such a series of operations of A forwards and of B backwards be continued, either alternately or simultaneously, there will result a continued production of work without any continued abstraction of heat from the source; and, by Prop. I., it follows that there must be more heat abstracted from the refrigerator by the working of B backwards than is deposited in it by A. Now it is obvious that A might be made to spend part of its work in working B backwards, and the whole might be made self-acting. Also, there being no heat either taken from or given to the source on the whole, all the surrounding bodies and space except the refrigerator might, without interfering with any of the conditions which have been assumed, be made of the same temperature as the source, whatever that may be. We should thus have a self-acting machine, capable of drawing heat constantly from a body surrounded by others at a higher temperature, and converting it into mechanical effect. But this is contrary to the axiom, and therefore we conclude that the hypothesis that A derives more mechanical effect from the same quantity of heat drawn from the source than B, is false. Hence no engine whatever, with source and refrigerator at the same temperatures, can get more work from a given quantity of heat introduced than any engine which satisfies the condition of reversibility, which was to be proved.

14. This proposition was first enunciated by Carnot, being the expression of his criterion of a perfect thermo-dynamic en-

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* If this axiom be denied for all temperatures, it would have to be admitted that a self-acting machine might be set to work and produce mechanical effect by cooling the sea or earth, with no limit but the total loss of heat from the earth and sea, or, in reality, from the whole material world.

gine*. He proved it by demonstrating that a negation of it would require the admission that there might be a self-acting machine constructed which would produce mechanical effect indefinitely, without any source either in heat or the consumption of materials, or any other physical agency ; but this demonstration involves, fundamentally, the assumption that, in "a complete cycle of operations," the medium parts with exactly the same quantity of heat as it receives. A very strong expression of doubt regarding the truth of this assumption, as a universal principle, is given by Carnot himself† ; and that it is false, where mechanical work is, on the whole, either gained or spent in the operations, may (as I have tried to show above) be considered to be perfectly certain. It must then be admitted that Carnot's original demonstration utterly fails, but we cannot infer that the proposition concluded is false. The truth of the conclusion appeared to me, indeed, so probable, that I took it in connexion with Joule's principle, on account of which Carnot's demonstration of it fails, as the foundation of an investigation of the motive power of heat in air-engines or steam-engines through finite ranges of temperature, and obtained about a year ago results, of which the substance is given in the second part of the paper at present communicated to the Royal Society. It was not until the commencement of the present year that I found the demonstration given above, by which the truth of the proposition is established upon an axiom (§ 12) which I think will be generally admitted. It is with no wish to claim priority that I make these statements, as the merit of first establishing the proposition upon correct principles is entirely due to Clausius, who published his demonstration of it in the month of May last year, in the second part of his paper on the motive power of heat‡. I may be allowed to add, that I have given the demonstration exactly as it occurred to me before I knew that Clausius had either enunciated or demonstrated the proposition. The following is the axiom on which Clausius' demonstration is founded :—

It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature.

It is easily shown, that, although this and the axiom I have used are different in form, either is a consequence of the other. The reasoning in each demonstration is strictly analogous to that which Carnot originally gave.

15. A complete theory of the motive power of heat would consist of the application of the two propositions demonstrated above, to every possible method of producing mechanical effect

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* Account of Carnot's Theory, § 13.   † Ibid. § 6.
  ‡ Poggendorff's *Annalen*, referred to above.

from thermal agency*. As yet this has not been done for the electrical method, as far as regards the criterion of a perfect engine implied in the second proposition, and probably cannot be done without certain limitations; but the application of the first proposition has been very thoroughly investigated, and verified experimentally by Mr. Joule in his researches " On the Calorific Effects of Magneto-Electricity;" and on it is founded one of his ways of determining experimentally the mechanical equivalent of heat. Thus, from his discovery of the laws of generation of heat in the galvanic circuit†, it follows that when mechanical work by means of a magneto-electric machine is the source of the galvanism, the heat generated in any given portion of the fixed part of the circuit is proportional to the whole work spent; and from his experimental demonstration that heat is developed in any moving part of the circuit at exactly the same rate as if it were at rest, and traversed by a current of the same strength, he is enabled to conclude—

(1.) That heat may be created by working a magneto-electric machine.

(2.) That if the current excited be not allowed to produce any other than thermal effects, the total quantity of heat produced is in all circumstances exactly proportional to the quantity of work spent.

16. Again, the admirable discovery of Peltier, that cold is produced by an electrical current passing from bismuth to antimony, is referred to by Joule‡, as showing how it may be proved

* "There are at present known two, and only two, distinct ways in which mechanical effect can be obtained from heat. One of these is by the alterations of volume which bodies experience through the action of heat; the other is through the medium of electric agency."—Account of Carnot's Theory, § 4. (Transactions, vol. xvi. part 5.)

† That, in a given fixed part of the circuit, the heat evolved in a given time is proportional to the square of the strength of the current, and for different fixed parts, with the same strength of current, the quantities of heat evolved in equal times are as the resistances. A paper by M. Joule, containing demonstrations of these laws, and of others on the relations of the chemical and thermal agencies concerned, was communicated to the Royal Society on the 17th of December 1840, but was not published in the Transactions. (See abstract containing a statement of the laws quoted above, in the Philosophical Magazine, vol. xviii. p. 308.) It was published in the Philosophical Magazine in October 1841 (vol. xix. p. 260).

‡ [In the introduction to his paper on the Calorific Effects of Magneto-electricity, &c., Phil. Mag. 1843.

I take this opportunity of mentioning that I have only recently become acquainted with Helmholtz's admirable treatise on the principle of mechanical effect (*Ueber die Erhaltung der Kraft*, von Dr. H. Helmholz. Berlin. G. Reimer, 1847), having seen it for the first time on the 20th of January of this year; and that I should have had occasion to refer to it on this, and on numerous other points of the dynamical theory of heat, the mechanical theory of electrolysis, the theory of electro-magnetic induction, and the

that, when an electrical current is continuously produced from a purely thermal source, the quantities of heat evolved electrically in the different homogeneous parts of the circuit are only compensations for a loss from the junctions of the different metals, or that, when the effect of the current is entirely thermal, there must be just as much heat emitted from the parts not affected by the source as is taken from the source.

17. Lastly*, when a current produced by thermal agency is made to work an engine and produce mechanical effect, there will be less heat emitted from the parts of the circuit not affected by the source than is taken in from the source, by an amount precisely equivalent to the mechanical effect produced; since Joule demonstrates experimentally, that a current from any kind of source driving an engine, produces in the engine just as much less heat than it would produce in a fixed wire exercising the same resistance as is equivalent to the mechanical effect produced by the engine.

18. The equality of thermal effects, resulting from equal causes through very different means, is beautifully illustrated by the following statement, drawn from Mr. Joule's paper on magneto-electricity†.

mechanical theory of thermo-electric currents, in various papers communicated to the Royal Society of Edinburgh, and to this Magazine, had I been acquainted with it in time.—W. T. March 20, 1852.]

* This reasoning was suggested to me by the following passage contained in a letter which I received from Mr. Joule on the 8th of July 1847. "In Peltier's experiment on cold produced at the bismuth and antimony solder, we have an instance of the conversion of heat into the mechanical force of the current," which must have been meant as an answer to a remark I had made, that no evidence could be adduced to show that heat is ever put out of existence. I now fully admit the force of that answer; but it would require a proof that there is more heat put out of existence at the heated soldering [or in this and other parts of the circuit] than is created at the cold soldering, [and the remainder of the circuit, when a machine is driven by the current,] to make the "evidence" be *experimental.* That this is the case I think is certain, because the statements of § 16 in the text are demonstrated consequences of the first fundamental proposition; but it is still to be remarked, that neither in this nor in any other case of the production of mechanical effect from purely thermal agency, has the ceasing to exist of an equivalent quantity of heat been demonstrated otherwise than theoretically. It would be a very great step in the experimental illustration (or *verification,* for those who consider such to be necessary) of the dynamical theory of heat, to actually show in any one case a loss of heat; and it might be done by operating through a very considerable range of temperatures with a good air-engine or steam-engine, not allowed to waste its work in friction. As will be seen in Part II. of this paper, no experiment of any kind could show a considerable loss of heat without employing bodies differing considerably in temperature; for instance, a loss of as much as ·098, or about one-tenth of the whole heat used, if the temperature of all the bodies used be between 0° and 30° Cent.

[† In this paper reference is made to his previous paper "On the Heat

Let there be three equal and similar galvanic batteries furnished with equal and similar electrodes; let A_1 and B_1 be the terminations of the electrodes (or wires connected with the two poles) of the first battery, A_2 and B_2 the terminations of the corresponding electrodes of the second, and A_3 and B_3 of the third battery. Let A_1 and B_1 be connected with the extremities of a long fixed wire; let A_2 and B_2 be connected with the "poles" of an electrolytic apparatus for the decomposition of water; and let A_3 and B_3 be connected with the *poles* (or *ports* as they might be called) of an electro-magnetic engine. Then if the length of the wire between A_1 and B_1, and the speed of the engine between A_3 and B_3, be so adjusted that the strength of the current (which for simplicity we may suppose to be continuous and perfectly uniform in each case) may be the same in the three circuits, there will be more heat given out in any time in the wire between A_1 and B_1 than in the electrolytic apparatus been A_2 and B_2, or the working engine between A_3 and B_3. But if the hydrogen were allowed to burn in the oxygen, within the electrolytic vessel, and the engine to waste all its work without producing any other than thermal effects (as it would do, for instance, if all its work were spent in continuously agitating a limited fluid mass), the total heat emitted would be precisely the same in each of these two pieces of apparatus as in the wire between A_1 and B_1. It is worthy of remark that these propositions are *rigorously* true, being demonstrable consequences of the fundamental principle of the dynamical theory of heat, which have been discovered by Joule, and illustrated and verified most copiously in his experimental researches[*].

19. Both the fundamental propositions may be applied in a

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of Electrolysis" (published in vol. vii. part 2, of the second Series of the Literary and Philosophical Society of Manchester) for experimental demonstration of those parts of the theory in which chemical action is concerned.]

[*] [I have recently met with the following passage in Liebig's Animal Chemistry (3rd edit. London, 1846, p. 43), in which the dynamical theory of the heat both of combustion and of the galvanic battery is indicated, if not fully expressed:—" When we kindle a fire under a steam-engine, and employ the power obtained to produce heat by friction, it is impossible that the heat thus obtained can ever be greater than that which was required to heat the boiler; and if we use the galvanic current to produce heat, the amount of heat obtained is never in any circumstances greater than we might have by the combustion of the zinc which has been dissolved in the acid."

A paper "On the Heat of Chemical Combination," by Dr. Thomas Woods, published last October in the Philosophical Magazine, contains an independent and direct experimental demonstration of the proposition stated in the text regarding the comparative thermal effects in a fixed metallic wire, and an electrolytic vessel for the decomposition of water, produced by a galvanic current.—W. T. March 20, 1852.]

perfectly rigorous manner to the second of the known methods of producing mechanical effect from thermal agency. This application of the first of the two fundamental propositions has already been published by Rankine and Clausius; and that of the second, as Clausius showed in his published paper, is simply Carnot's unmodified investigation of the relation between the mechanical effect produced and the thermal circumstances from which it originates, in the case of an expansive engine working within an infinitely small range of temperatures. The simplest investigation of the consequences of the first proposition in this application, which has occurred to me, is the following, being merely the modification of an analytical expression of Carnot's axiom regarding the permanence of heat, which was given in my former paper[*], required to make it express, not Carnot's axiom, but Joule's.

20. Let us suppose a mass[†] of any substance, occupying a volume v, under a pressure p uniform in all directions, and at a temperature t, to expand in volume to $v + dv$, and to rise in temperature to $t + dt$. The quantity of work which it will produce will be

$$vdv;$$

and the quantity of heat which must be added to it to make its temperature rise during the expansion to $t + dt$ may be denoted by

$$\mathrm{M}dv + \mathrm{N}dt.$$

The mechanical equivalent of this is

$$\mathrm{J}(\mathrm{M}dv + \mathrm{N}dt),$$

if J denote the mechanical equivalent of a unit of heat. Hence the mechanical measure of the total external effect produced in the circumstances is

$$(p - \mathrm{JM})dv - \mathrm{JN}dt.$$

The total external effect, after any finite amount of expansion, accompanied by any continuous change of temperature, has taken place, will consequently be, in mechanical terms,

$$\int \{ (p - \mathrm{JM})dv - \mathrm{JN}dt \};$$

where we must suppose t to vary with v, so as to be the actual temperature of the medium at each instant, and the integration with reference to v must be performed between limits corresponding to the initial and final volumes. Now if, at any subsequent time, the volume and temperature of the medium become what they were at the beginning, however arbitrarily they may have been made to vary in the period, the total external effect must,

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[*] Account of Carnot's Theory, foot-note on § 26.

[†] This may have parts consisting of different substances, or of the same substance in different states, provided the temperature of all be the same. See below, part 3, § 53–56.

according to Prop. I., amount to nothing; and hence
$$(p - JM)dv - JN dt *$$
must be the differential of a function of two independent variables, or we must have
$$\frac{d(p - JM)}{dt} = \frac{d(-JN)}{dv}, \quad . \quad . \quad . \quad . \quad (1)$$
this being merely the analytical expression of the condition, that the preceding integral may vanish in every case in which the initial and final values of v and t are the same, respectively. Observing that J is an absolute constant, we may put the result into the form
$$\frac{dp}{dt} = J\left(\frac{dM}{dt} - \frac{dN}{dv}\right). \quad . \quad . \quad . \quad . \quad (2)$$
This equation expresses, in a perfectly comprehensive manner, the application of the first fundamental proposition to the thermal and mechanical circumstances of any substance whatever, under uniform pressure in all directions, when subjected to any possible variations of temperature, volume and pressure.

21. The corresponding application of the second fundamental proposition is completely expressed by the equation
$$\frac{dp}{dt} = \mu M, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$
where μ denotes what is called "Carnot's function," a quantity which has an absolute value, the same for all substances for any given temperature, but which may vary with the temperature in a manner that can only be determined by experiment. To prove this proposition, it may be remarked in the first place that Prop. II. could not be true for every case in which the temperature of the refrigerator differs infinitely little from that of the source, without being true universally. Now, if a substance be allowed first to expand from v to $v + dv$, its temperature being kept constantly t; if, secondly, it be allowed to expand further, without either emitting or absorbing heat till its temperature goes down through an infinitely small range, to $t - \tau$; if, thirdly, it be compressed at the constant temperature $t - \tau$, so much (actually by an amount differing from dv by only an infinitely small quantity of the second order), that when, fourthly, the volume is further diminished to v without the medium's being allowed to either emit or absorb heat, its temperature may be exactly t; it may be considered as constituting a thermo-dynamic

[* The integral function $\int\{(JM - p)dv + JN dt\}$ may obviously be called the *mechanical energy* of the fluid mass; as (when the constant of integration is properly assigned) it expresses the whole work the fluid has in it to produce. The consideration of this function is the subject of a short paper communicated to the Royal Society of Edinburgh, Dec. 15, 1851, as an appendix to the paper at present republished.]

engine which fulfills Carnot's condition of complete reversibility.
Hence, by Prop. II., it must produce the same amount of work
for the same quantity of heat absorbed in the first operation, as
any other substance similarly operated upon through the same
range of temperatures. But $\frac{dp}{dt}\tau.dv$ is obviously the whole work
done in the complete cycle, and (by the definition of M in § 20)
Mdv is the quantity of heat absorbed in the first operation.
Hence the value of

$$\frac{\frac{dp}{dt}\tau.dv}{\mathrm{M}dv}, \quad \text{or} \quad \frac{\frac{dp}{dt}}{\mathrm{M}}\tau,$$

must be the same for all substances, with the same values of t
and τ; or, since τ is not involved except as a factor, we must have

$$\frac{\frac{dp}{dt}}{\mathrm{M}}=\mu, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where μ depends only on t; from which we conclude the pro-
position which was to be proved.

22. The very remarkable theorem that $\frac{\frac{dp}{dt}}{\mathrm{M}}$ must be the same
for all substances at the same temperature, was first given
(although not in precisely the same terms) by Carnot, and de-
monstrated by him, according to the principles he adopted. We
have now seen that its truth may be satisfactorily established
without adopting the false part of his principles. Hence all Car-
not's conclusions, and all conclusions derived by others from his
theory, which depend merely on equation (3), require no modifi-
cation when the dynamical theory is adopted. Thus, all the
conclusions contained in Sections I., II., and III., of the Ap-
pendix to my Account of Carnot's Theory, and in the paper im-
mediately following it in the Transactions, entitled "Theoretical
Considerations on the Effect of Pressure in Lowering the Freezing
Point of Water," by my elder brother, still hold. Also, we see
that Carnot's expression for the mechanical effect derivable from
a given quantity of heat by means of a perfect engine in which
the range of temperatures is infinitely small, expresses truly the
greatest effect which can possibly be obtained in the circum-
stances; although it is in reality only an infinitely small fraction
of the whole mechanical equivalent of the heat supplied; the
remainder being irrecoverably lost to man, and therefore "wasted,"
although not *annihilated.*

23. On the other hand, the expression for the mechanical
effect obtainable from a given quantity of heat entering an engine

from a "source" at a given temperature, when the range down to the temperature of the cold part of the engine or the "refrigerator" is finite, will differ most materially from that of Carnot; since, a finite quantity of mechanical effect being now obtained from a finite quantity of heat entering the engine, a finite fraction of this quantity must be converted from heat into mechanical effect. The investigation of this expression, with numerical determinations founded on the numbers deduced from Regnault's observations on steam, which are shown in Tables I. and II. of my former paper, constitutes the second part of the paper at present communicated.

[To be continued.]

III. *On the Fatty Acid of* Cocculus indicus.
By Mr. W. Crowder, *Assistant to* Dr. Anderson *of Edinburgh*[*].

IN a paper published several years ago in the *Annalen der Chemie und Pharmacie*[†] upon the substances obtained from *Cocculus indicus,* Dr. Francis pointed out the existence of a fatty acid which had not previously been subjected to investigation, to which he gave the name of stearophanic acid; and after analysing it, its æther and silver salt, he gave the formula $C^{35}H^{35}O^4$ as representing its constitution. The recent researches of chemists having pointed out that all the fatty acids have the same general formula $(C^2H^2)^nO^4$, and the acid under consideration being evidently a member of that series, it seemed probable that its true formula would be $C^{36}H^{36}O^4$, with which the results obtained by Dr. Francis, when calculated with the corrected atomic weight of carbon, closely agree. This formula has indeed been since assumed by Dr. Francis himself and by other chemists, as the true expression of his results; and as the investigation of the fatty acids has since the date of his experiments made great strides, I thought it desirable to submit the fat of *Cocculus indicus* to a new examination, the result of which has fully confirmed the correctness of Dr. Francis's numbers, and conclusively established the formula $C^{36}H^{36}O^4$, and consequently the identity of the acid with the bassic acid since described by Mr. Hardwicke[‡]. My experiments were performed in the labora-

tory of Dr. Anderson, for whose kindness and valuable sugges-
tions I am greatly indebted.

In order to obtain the substance in sufficient quantity for
experiment and for thorough purification, I commenced upon
fourteen pounds of the berries. The kernels (which contain the
fat) were separated by cracking the shells and picking them out
with a pointed instrument, a process sufficiently troublesome and
involving a great loss of time. The kernels were next beaten
into a paste in a warm mortar, and after being heated for some
time at 212° in order to melt the fat, they were subjected to
hydraulic pressure between two plates of hot lead. In a short
time a great quantity of perfectly colourless oil made its appear-
ance, which upon cooling solidified into a mass resembling stea-
rine. The residue in the cloths, being reheated and pressed a
second time, gave a small additional quantity of fat.

The quantity of kernels obtained from the berries amounted
to 28 per cent. The quantity of fat obtained from the berries
amounted to $15\frac{1}{2}$ per cent.

The fat is exceedingly soluble in æther, sparingly soluble in
absolute alcohol, and almost insoluble in rectified spirit. It begins
to melt at 72° F., but is not completely fused till the temperature
rises to 78° F. Like other fats, it crystallizes in warty masses
from its solution in hot æther, and in an arborescent form when
its hot alcoholic solution is cooled. The fat was saponified with
caustic soda, and the soap formed separated from the solution
by common salt. The soap was allowed to become hard by cool-
ing, repeatedly mixed with small quantities of water to wash
away the salt, the soap pressed, dissolved in water, and decom-
posed by sulphuric or hydrochloric acid. Upon cooling, the acid
was subjected to very cautious pressure in order to separate it as
far as possible from oleic acid, which was present in consider-
able quantity. The product was then crystallized from alcohol
till its fusing-point became constant at 159° F.

The acid when pure is highly crystalline ; it melts at 159° F.,
and, like *all* the acids of the fatty series, is volatile to a certain
extent without decomposition.   It reddens litmus distinctly,
and decomposes the alkaline carbonates with effervescence when
boiled along with them.   It is exceedingly soluble in hot alcohol
and in æther, and from its solution in the former liquid it is
almost entirely separated on cooling.

The accurate analysis of this substance was attended with some
degree of difficulty. In the earlier analyses, in which oxide of
copper alone was used, results were obtained which in all but
one instance gave the per-centage of carbon decidedly too low.
In my subsequent analyses I therefore used from ten to fifteen
grains of chlorate of potash with my oxide of copper or chromate

of lead, and then succeeded in obtaining results closely corresponding to the calculated numbers. The following were the results :—

I. 4·49 grains of acid, burned with oxide of copper *alone*, gave 12·49 carbonic acid and 5·35 water.

II. 4·315 grains of acid, burned with oxide of copper and chlorate of potash, gave 12·080 carbonic acid and 5·455 water.

III. 4·345 grains of acid, burned with chromate of lead and chlorate of potash, gave 12·165 carbonic acid and 5·088 water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon .	75·86	76·33	76·34	76·05	C^{36}	216
Hydrogen	13·22	14·04	13·01	12·67	H^{36}	36
Oxygen .	10·92	9·63	10·65	11·28	O^4	32
	100·00	100·00	100·00	100·00		284

In No. II. the hydrogen is evidently too high, arising from the necessity of mixing the substance with the oxide nearly cold. These results agree sufficiently well with the formula $C^{36}H^{36}O^4$, which is that of bassic acid, and were further confirmed by the analysis of its æther.

Bassic Æther.—This compound was prepared by passing a current of dry hydrochloric acid gas into a solution of the acid in alcohol, taking care to keep the fluid quite hot during the first part of the operation, since the acid would otherwise crystallize out. After the lapse of some time an oily and colourless liquid floats to the top, which on cooling concretes into a brittle crystalline mass. This is the æther which must be washed with water to free it from hydrochloric acid, and crystallized from alcohol once or twice to free it entirely from any adhering fatty acid. It is moderately soluble in hot alcohol, very sparingly so in the cold, and is deposited from its hot alcoholic solution upon cooling in needles. It fuses at 92°, and is slightly volatile when kept in the water-bath at 212°. It is without smell, and when placed on the tongue it melts, producing a slight sensation of cold.

The same difficulty of combustion was observed in burning this substance as in the case of the acid. Of six combustions made with oxide of copper alone, only two gave the theoretical result for the carbon, the other four gave numbers evidently too low. I therefore made one combustion with chromate of lead and chlorate of potash, in order to control the two analyses which gave higher per-centages of carbon.

The following are the details :—

I. 4·230 grains of the æther, dried *in vacuo* and burned with oxide of copper, gave 11·945 carbonic acid and 5·000 water.

II. 4·835 grains of the æther, dried at 212° and burned with oxide of copper, gave 13·645 carbonic acid and 5·850 water.

III. 4·555 grains of the æther, fused during a quarter of an hour at 212° and burned with chromate of lead and chlorate of potash, gave 12·815 carbonic acid and 5·372 water.

		Experiment.			Calculation.	
	I.	II.	III.			
Carbon .	77·01	76·96	76·73	76·95	C^{40}	240
Hydrogen	13·13	13·44	13·11	12·82	H^{40}	40
Oxygen .	9·86	9·60	10·16	10·23	O^4	32
	100·00	100·00	100·00	100·00		312

These results correspond exceedingly well with the formula $C^{36}H^{35}O^3 + C^4H^5O = C^{40}H^{40}O^4$, or the acid combined with one atom of oxide of æthyle.

Bassiate of Potash.—This salt was prepared by dissolving the acid in an aqueous solution of boiling carbonate of potash, evaporating to dryness and taking up with strong alcohol, when the carbonate of potash in excess is left behind. The salt on cooling separates as a jelly, which may be freed from alcohol by squeezing. It is then redissolved in alcohol, allowed to cool and squeezed a second time, and the purification is complete. It is exceedingly soluble in hot alcohol, from which it separates as a jelly when allowed to cool; it is also soluble in æther when a very small quantity of alcohol is added, and from this solution crystallizes in needles when allowed to evaporate spontaneously.

Bassiate of Soda.—This salt is prepared in exactly the same manner as the preceding, substituting carbonate of soda for carbonate of potash. When dry and in masses, it has a shining semi-crystalline appearance; but when in powder, it is destitute of all appearance of crystallization, even under the microscope. It is insoluble in æther, soluble in alcohol, from which it separates almost entirely as an opake jelly on cooling, scattered through which may sometimes be observed a few minute needle-like crystals; but my attempts at obtaining a regular crop of crystals were entirely unsuccessful.

This substance is also soluble in boiling-hot water, from which it again separates as a jelly on cooling. A large addition of hot water to the solution of this salt causes it to become opake, arising no doubt from decomposition.

The determinations of soda are as follows:—

I. 6·33 grains of soda salt, ignited and afterwards treated with sulphuric acid, gave 1·455 sulphate of soda = 10·35 per cent. of soda. 6·225 grains of soda salt, treated as before, gave 1·450 sulphate of soda = 10·17 per cent. of soda.—Mean = 10·26.

II. 4·935 grains of soda salt, treated as before, gave 1·135 sulphate of soda = 10·03 per cent. of soda. 5·35 grains of soda salt gave, on ignition, 0·92 of carbonate of soda = 10·05 per cent. of soda.—Mean 10·04.

		Experiment.	Mean of four determinations.	Calculation.	
Carbon	. . .	...	70·58	C^{36}	216
Hydrogen	. . .	...	11·43	H^{35}	35
Oxygen	. . .	...	7·86	O^3	24
Soda	. . .	10·15	10·13	NaO	31
		...	100·00		306

No. I. was prepared by adding an alcoholic solution of caustic soda to an alcoholic solution of the acid. The salt separates on cooling, and is to be purified by re-solution in rectified spirit.

No. II. was prepared in the same manner as the potash salt, substituting carbonate of soda for carbonate of potash.

So far as the determination of soda in this substance is concerned, it may be inferred that the formula given is correct; but as the combustion required the use of chlorate of potash, I did not deem it necessary to incur the trouble of performing that operation.

Bassiate of Ammonia.—This salt may be prepared by dissolving the acid in a hot dilute solution of caustic ammonia and allowing it to cool, when it crystallizes in innumerable small needles. A clear solution of this salt, if kept hot for any length of time, becomes opalescent from decomposition; and even the addition of a fresh quantity of ammonia does not cause the precipitate, once formed, to redissolve entirely. It is soluble in alcohol and in æther, and is very prone to decomposition from loss of ammonia.

Bassiate of Baryta.—This salt was prepared and purified with very great ease by dissolving the acid in a small excess of caustic ammonia, and whilst still quite hot, adding a solution of chloride of barium. A white curdy precipitate is formed, which is to be filtered and washed with boiling water till free from chloride of barium, and dried at 212°. It is a white powder, without any apparent crystalline structure, insoluble in water, alcohol, and æther of high specific gravity.

Analysis gave the following results:—

I. 6·460 grains of baryta salt gave, on ignition, 1·810 of carbonate of baryta = 21·74 per cent. baryta.

II. 5·820 grains of baryta salt gave, on ignition, 1·635 of carbonate of baryta = 21·82 per cent.

5·865 grains of baryta salt gave 13·270 of carbonic acid and 5·445 of water.

	Experiment.		Calculation:		
	I.	II.			
Carbon . .	61·68	...	61·44	C^{36}	216
Hydrogen .	10·31	...	9·95	H^{35}	35
Oxygen . .	6·27	...	6·84	O^3	24
Baryta . .	21·74	21·82	21·77	BaO	76·55
	100·00		100·00		351·55

These numbers correspond closely with the theoretical result, leading to the formula $C^{36}H^{35}O^3 + BaO$.

Bassiate of Silver.—I next attempted the preparation of a silver salt, and after three or four trials, found that the best method of preparing it free from excess of acid was to make an exceedingly dilute solution of soda in alcohol, and also an exceedingly dilute solution of nitrate of silver in the same menstruum.

The proportions used were about 20 grains of soda salt dissolved in 5 or 6 ounces of rectified spirit, and about 12 or 13 grains of fused nitrate of silver also dissolved in the same quantity of alcohol. The solution of soda salt was added to the solution of nitrate of silver in small successive quantities with vigorous stirring in the intervals, and the mixture was made in the cold; since I found that, by mixing them together hot, more or less decomposition invariably took place. The white curdy precipitate is allowed to subside in the dark, the supernatant liquid is drawn off, and the precipitate filtered in the dark, as the salt blackens by exposure to the light when alcohol is present. It was then washed with alcohol till free from excess of silver, dried first *in vacuo* and then at 212°.

Thus prepared, it is a light white powder, without any appearance of crystallization; it is highly electrical, insoluble in water, alcohol, and in æther. It speedily blackens in contact with alcohol, but when freed from that liquid it undergoes decomposition much more slowly.

The following are the results of analysis :—

I. 6·258 grains of silver salt, prepared as above detailed, gave, on ignition, 1·733 silver = 27·69. 6·825 grains of silver salt, of the same preparation, gave 1·875 silver = 27·46.—Mean 27·57.

II. 6·515 grains of silver salt, prepared with aqueous solutions, gave 1·825 silver = 28·01 per cent. 5·66 grains of silver salt, of the same preparation, gave 1·58 silver = 27·91 per cent. —Mean 27·96.

III. (*a*) 4·93 grains of silver salt, prepared as No. I., gave 1·35 silver = 27·38 per cent. (*b*) 4·922 grains of silver salt, of the same preparation, gave 1·346 silver = 27·34 per cent.—Mean 27·36.

	Experiment.				Calculation.		
	I.	II.	III.				
			a.	*b.*			
Carbon .	...	55·90	55·33	55·37	55·24	C^{36}	216
Hydrogen	...	9·03	9·27	10·10	8·95	H^{35}	35
Oxygen .	...	7·11	8·03	7·17	8·19	O^{4}	32
Silver . .	27·57	27·96	27·36	27·36	27·62	Ag	108
	100·00	100·00	100·00	100·00	100·00		391

a was burned with chromate of lead; *b* was burned with oxide of copper; the excess of hydrogen arose from an accident in the laboratory during the time of mixing. The formula of this substance agrees quite well with $C^{36}H^{35}O^{3} + AgO$.

The evidence of this substance being bassic acid appears by the preceding experiments to be in every way complete, and also exceedingly interesting, since the occurrence of the acid has been clearly made out in two entirely distinct natural orders of plants. Possibly, by a more rigorous search among the acids contained in the fatty matters of plants, this substance will be found to be much more widely distributed than has hitherto been supposed; and thus the loose statements of the occurrence of stearic acid in vegetables, without any analysis appended, may be proved incorrect by demonstrating the existence of bassic acid instead.

Besides the acid just described, the fat contains a very considerable proportion of oleic acid, or at all events of an oily acid, and a quantity of another fatty acid which I have not attempted to isolate, but which appears to be the same as one also observed by Mr. Hardwicke in *Bassia latifolia*. He says, that in the fat of that plant there are two acids present, viz. bassic acid, and another having a melting-point between 132° and 134°; and that if a drop of an alcoholic solution of this last acid be allowed to evaporate on the surface of a glass plate so as to form a thin film, it presents, on solidifying, the curious appearance of a series of concentric rings, which may not unaptly be compared to a section of bone under the microscope; and that this appearance is also seen when the bassic acid is impure, but it disappears when completely purified.

These appearances have been observed by myself in the acid from *Cocculus indicus* when it has been melted in the water-bath and allowed to cool; and from it I conclude that the specimen must have been contaminated with the acid melting at 132° to 134°, and the formula of which is either $C^{30}H^{30}O^{4}$, the missing member of the fatty series, or that of palmitic acid, $C^{32}H^{32}O^{4}$.

I shall conclude this communication by appending a tabular view of all the combinations I have examined.

Bassic acid . . .	$C^{36}H^{35}O^3 + HO$.
Bassic æther . . .	$C^{36}H^{35}O^3 + C^4H^5O$.
Bassiate of soda .	$C^{36}H^{35}O^3 + NaO$.
Bassiate of baryta .	$C^{36}H^{35}O^3 + BaO$.
Bassiate of silver .	$C^{36}H^{35}O^3 + AgO$.

IV. *Additions to the late* Mr. T. S. Davies's *Notes on Geometry and Geometers. The Swale Manuscripts. By* T. T. WIL-KINSON, *Esq., F.R.A.S.**

WHEN Professor Davies wrote No. VI. of his "Geometry and Geometers" (Phil. Mag. Sept. 1850), he adopted an opinion of mine, in correction of his own, to the effect that Mr. William Chapple was the first English geometer who had formally stated the property, that " the perpendiculars from the angles of a plane triangle on the opposite sides intersect in the same point." Since that time, I have been led to examine the matter more fully, and in two papers printed in the Mechanics' Magazine, Nos. 1430 and 1458, I have shown that not only was the property published in the "Miscellanies, or Mathematical Lucubrations of Mr. Samuel Foster, sometime Publike Professor of Astronomie in Gresham Colledge in London, 1659," where it is annexed to a commentary on the "Lemmata Archimedis" by the Arabian commentator Abi Alhonin Ali, but that it was un-doubtedly known to the ancients, since it is implied in and fol-lows as an easy inference from "Theorema LVII., Propositio LX." of "Pappi Alexandrini Mathematicæ Collectiones. A. Frederico Commandino. Venetiis 1589, folio 195 *b* ;" and that from these sources probably those geometers derived the property previously to its being formally enunciated by Mr. Chapple in the *Mis-cellanea Mathematica.* The note to No. VI., page 208, therefore requires correction, and before entering upon other matters it will perhaps not be out of place if I notice one or two other over-sights which elsewhere occur in this singularly exact and inter-esting series of papers.

In No. VIII., the first issue of the "Mathematical Repository" is put down as "March 1, 1796," and this date appears to have been deduced by reckoning backwards two half-years from the date of the publication of the third number. The excellent prac-tice of binding up the covers of each number with each volume of periodical works, seems to have been partially followed in the copy from which Professor Davies quotes, *but the title-page printed along with the first number of the work* must have been omitted.

In my copy the full title runs thus :—" The Mathematical Repository : containing many ingenious and useful Essays and Extracts, with a collection of Problems and Solutions, selected from the correspondence of several able Mathematicians, and the works of those who are eminent in the Mathematics. London, 1795 ;" and as all " Letters for the use of No. II. . . . . . . must come to hand before the first of January 1796," it seems almost certain that the first number was published about the beginning of October, 1795.

The "Mathematical Companion" was projected by several active members of the Spitalfields Mathematical Society, and, with the exception of the last number, was edited by its members Messrs. Davis and Hampshire from its commencement in November 1797, to its close in November 1826. On its projection it was proposed to designate it " A Companion to the Gentleman's Diary, or a Preparation to that useful work," and the first number was actually printed with that title ; but since the *then* Editor of the "Gentleman's Diary," the Rev. Charles Wildbore (*not Dr. Hutton*), had not been consulted respecting the new publication, he declared in the Diary for 1798 that he "would discourage it all in his power," and consequently the *second* number was issued as the "Gentleman's Mathematical Companion," to which the title of the first number was altered on its being reprinted in 1809. The reasons for this change are fully stated in the "Advertisement" prefixed to the reprint of 1809, and must have been overlooked by Professor Davies when writing the remarks contained in "Geometry and Geometers, No. VIII." The "open field" alluded to is also liable to correction, for Mr. Whiting's "Scientific Receptacle" and "Geometrical Delights" had appeared at intervals for several years previously to the appearance of the Repository, Mr. Leybourn himself being a joint proprietor in the latter work with Messrs. Whiting and Davis.

The whole of No. VII. of this series of papers is devoted to an analysis of Mr. Swale's merits as a geometer, and to a short notice of his "Geometrical Amusements," the "Apollonius," and his manuscript remains. Had the hand of death not so prematurely arrested his progress, we should have had much more on this interesting topic, since the MSS. could have furnished much available matter for Mr. Davies's versatile and discursive genius ; and it is much to be regretted that the task, if ever it be accomplished, should have devolved upon others so much less able and experienced. Most of the inferences and deductions respecting Mr. Swale's speculations in mathematical publications, &c. have been confirmed to me in private correspondence with his son, the present Mr. J. H. Swale ; nor will any one who is at all acquainted with his father's writings, hesitate for a

moment to accept Mr. Davies's estimate of his abilities as a
geometer in the fullest acceptation of the terms ; but with regard
to existing manuscripts, it may be observed that the following are
in existence "of a date prior to 1828," which, however, I have
not yet seen, nor were they sent by Mr. Swale amongst the rest
for Mr. Davies's inspection.

No. I.	Geometrical Disquisitions.	Christmas.	1811.
II.	Geometrical Amusements.	Christmas.	1818.
III.	,,	,,	$\left\{\begin{array}{l}\text{Midsummer,}\\\text{Christmas.}\end{array}\right\}$ 1819.
IV.	,,	,,	Christmas. 1820.
V.	Geometrical Sketches.	$\left\{\begin{array}{l}\text{Midsummer.}\\\text{Christmas.}\\\text{Christmas.}\end{array}\right.$	$\left.\begin{array}{l}1823.\\1823.\\1824.\end{array}\right\}$
VI.	Geometrical Papers.		

Several of the other manuscripts bear evidences of having been
transcribed from some of older date, since they contain original
investigations which had previously appeared in different peri-
odicals, and amended solutions to questions which had been
proposed in " Burrows's Diary," and various other mathematical
publications of the last and present centuries.   This is more
particularly the case with two of the oldest MSS. ; the rest appear
to have resulted from his practice of " spinning geometrical cob-
webs" as an *amusement* during the leisure hours of declining age.
Up to the time when the " Geometrical Amusements" were pub-
lished in 1821, no attempts had been made to improve the style
of printing geometrical demonstrations : the old hackneyed form
had been rigidly adhered to by both editor and author, nor had
any geometer appeared who had ventured to deviate from the
established usage of printing entirely across the page.   Mr. Swale,
however, had learnt that the *eye* had something to do in geometry
as well as the intellect, and in his anxiety to aid *both*, he adopted,
to a considerable extent, the practice of printing each step in a
separate line, which has since been so fully carried out by Mr.
Potts in his excellent editions of Euclid's Elements.   The pages
of the " Amusements" therefore presented a somewhat novel
aspect to the mathematicians of his acquaintance, and this,
rather than his practice of making verses, induced them to banter
him respecting his " poetical geometry."

The " Geometry of the Circle," or as *his son* has not unaptly
endorsed the manuscript volumes, the " *Mascheronian Geometry*,"
had peculiar attractions for him :—several of the MSS. contain
short tracts on the subject, but volumes VIII. and IX. are wholly
devoted to its consideration.   Mascheroni's *Géométrie du Compas*
was for a long time his favourite work, and is contained in a list

of mathematical treatises which he was in the habit of taking along with him when he set out on his annual excursions into the country. The results of his study are the two manuscript volumes already noticed, both of which contain numerous interesting extensions of the use of the circle in geometrical constructions, and many examples of the highest ingenuity in its application. No. I., as Mr. Davies terms it, or No. VIII. according to Mr. Swale's enumeration, commences with the division and subdivision of lines, the division of arcs of circles, the drawing of common tangents, and finding proportionals. He then proceeds to the description of polygons, their inscription in circles and in each other, the inscription and circumscription of circles in triangles, &c., to many of which four or five different methods of construction are given. "Fertility in resource is increased power" was ever his favourite maxim, and throughout the whole of his writings he has endeavoured fully to illustrate its truth. No. II., or more correctly No. IX., is by far the most curious and valuable. He commences with the problems of having " given three or four tangential circles inscribed in a given circle, to describe another circle that shall touch the given one and any two of the inscribed circles," and after having given elegant constructions to these, he proceeds to the construction of the various cases of the Apollonian problem of tangencies, with the exception of that where a tangent circle to three given circles is required to be described, the *enunciation* only of which is given. Professor Davies regrets this circumstance, owing to the " probability that had Mr. Swale succeeded in this, it might have opened the road to a new system of treatment of the general problem ;"—but if we are to be guided in our conjectures by what is already done in the MS. with respect to the subsidiary problem of " describing through a given point, a circle which shall touch two given circles," to which the case of a tangent circle to three given ones may always be reduced, we may safely infer that Mr. Swale had obtained no clue to any *essentially new* process for the general case left unconstructed. In his valuable paper " On Tangential Circles" printed in the first number of the " Apollonius," he considers the cases when the three given circles " *touch each other*," and when they are " *anyhow posited* ;" both cases are constructed solely from the properties of what are now termed the poles of similitude, the first agreeing in principle with the construction given in Anderson's " Variorum Problematum Practice," of which a translation accompanies the construction in the " Apollonius," and the second reducing it by means of the same properties to the subsidiary problem previously noticed. The enunciation and construction of the subsidiary problem itself are given in the manuscript, as follows :—

" *Problem.*—A point P, and two circles, radii AT, BV, are given in position ; to describe a circle through P, that shall touch the given circles.

" *Construction* I.—Draw the tangents PM, PN ; take AH : HB = AM : BN, and AI : IB = PM² : PN² ; to the circle centre I and radius a fourth proportional to HB, HI, BN ; draw the tangent PR, and let the direction RH meet the circles in K, L ; then PL, PK, will meet them in T, V, the required points of contact.

" *Construction* II.—Take AH : HB = AT : BV ; inflect the tangent HR (in the arc through H to centre P) to I and K ; to which centres and radius IH, describe arcs intersecting in Q : the circle through the points P, Q, to touch the circle AT, is the one required."—(*MS.* pages 113–4.)

The first of these constructions is *identical* with that given by M. Cauchy in the *Correspondance sur l'Ecole Polytechnique*, vol. i., a translation of which may be seen in Leybourn's " Ladies' Diaries," vol. iv. pp. 269, 270, whose process has been elegantly extended to the general case of tangencies by " Centurion" in No. 1154 of the Mechanics' Magazine, and which again is almost identical with a construction given by Mr. Swale himself in MS. vol. ii. p. 384. The second construction is derived from a discussion of the tangencies contained in pp. 383–6 of the volume just cited, where the whole are most ingeniously reduced on Simpson's principles (Select Exercises, Prob. 57), to the subsidiary problem of determining " a point in a right line given in position, such, that lines drawn thence to two given points may have a given difference." The remainder of the volume is occupied with the construction of numerous other problems relating to the intersection of circles, tangents to them from given points or in given ratios, many of which are equally curious and interesting.   A remarkably neat construction of the problem, " to describe a circle that shall bisect the circumferences of two given circles, centres A and B, and have a tangent from a given point D of a given length P," is given in page 141 * ; and also the construction of a fourth circle " to bisect the circumferences of three given ones" in page 144, which has been published as Quest. 343 of the " Educational Times." His objects throughout appear to have been to extend and diversify Mascheroni's methods, for he remarks at the foot of a construction which closes the volume, " I find this is similar to Mascheroni's," and in these respects he has succeeded to a greater extent than

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* *Construction.*—Posite the diameter FG of the circle (A), and the radius BL of the circle (B) perpendicular to the direction AB, which meets the circle FGL in P and Q; inflect the line P (in the arc through D to centre P) to I and K ; to which centres and radius ID describe arcs intersecting in R : the circle PQR is the one required.

it is possible for any verbal statement to describe.   It does not appear that Mr. Swale ever *actually wrote out for the press* those portions of his geometrical sketches intended for Parts II. and III., of his " Geometrical Amusements," although the MSS. contain much available matter, which, to him, would have required little more than transcription and arrangement.   The titles of the manuscripts previously enumerated have no reference to their intended destination, but are adopted as somewhat indicative of the geometrical character of their contents ; he did not hesitate to term those *amusements* which to others less gifted would be found a severe mental exercise.   Even were this not so, all further speculations in the way of publication would have been effectually stopped by the losses attendant upon the unsaleable character of " Part I."; the reactionary taste for the Geometry of Coordinates had already been created, and the Ancient Analysis, unable to cope with this more powerful instrument of research, rapidly sunk into disuse : the " Geometrical Amusements" were of too antiquated a cast to secure many purchasers, nor did the " Apollonius" secure a better fate :—the *first* number " did not pay," whilst the *second* proved almost " a dead failure," partly from the above causes, but principally, as Mr. Marrat informs me, from Mr. Swale's admitting into its pages a long and intemperate attack upon the Newtonian System of Astronomy, by his friend Bartholomew Prescot.

Burnley, Lancashire.
Feb. 28th, 1852.

V. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electromagnetism, Magneto-electricity, and Thermo-electricity.   By* M. Donovan, *Esq., M.R.I.A.*

Section VI.

I NOW proceed to the consideration of another alleged proof of this identity, found in the magnetic properties known to be exercised by common and voltaic electricity.   Notwithstanding the difficulty of collecting the precise opinions of philosophers concerning the mutual dependence on each other of magnetism and electricity, loosely expressed as they sometimes are, it will probably be a safe enunciation to say, that by some, the two powers are supposed to be identical ; by others, that being each *sui generis* they reproduce each other ; by others, that although different they are always concurrent ; while others speak obscurely about " the current" and its power of producing magnetism, the

term current being used in a sense which I conceive cannot be reconciled with the laws of electricity.

Without examining these opinions separately and in any particular order, it will answer the purpose and save trouble to make mixed observations calculated to bring what I conceive to be their defects under observation.

Whatever difference of opinion may exist relative to the nature of positive and negative electricity, whether they are states of one fluid, or two distinct fluids, or vibrations of a peculiar fluid or of matter, I believe it is a position universally agreed to amongst electricians, that when equal to each other and at liberty to act, they mutually neutralize and destroy each other's properties. To quote authorities would be to enumerate all the authors who have written on the subject, I shall merely quote the expression of the fact as stated by Sir H. Davy :—" In all cases of electrical action, the two electrical states are always coincident, either in different parts of the same body, or in two bodies, and they are always equal and capable of neutralizing each other. If a connexion be made by a wire between the positive and negative conductors of the electrical machine during the time of its action, all electrical effects cease*." Instances without number might be adduced in support of the truth of this position ; but to proceed with them would be to prove what nobody doubts. Universally, if the two electricities be equal in quantity and intensity and are at perfect liberty to neutralize each other, all symptoms of both disappear, a condition of absolute quiescence results, that of equilibrium, in which all bodies naturally exist, is induced ; and in this state they manifest no electrical properties.

The poles of a voltaic series being in the positive and negative states, conform to the general law. When the poles are unconnected, they manifest their electrical condition to a gold-leaf electrometer ; but as soon as they are connected by a good conductor, the positive and negative states and all symptoms of electricity vanish.

This is a fact, which, so far as electrical appearances are concerned, is universally admitted ; yet it may not be without use to advert to the very striking exemplification of it lately given by Mr. Gassiot, on a scale of expense and magnificence rarely equalled by an individual. With a water battery consisting of 3520 pairs of copper and zinc cylinders, each pair being placed in a separate glass vessel well varnished, Mr. Gassiot made the following observation :—" The tension was so great that the gold leaves of an electroscope diverged when that instrument was

* Elements of Chemical Philosophy, p. 132. The thickness of the wire must be proportional to the quantity to be conducted.

placed within two or three inches of either end of the battery, or over any of the terminal cells. Advantage was taken of this to test whether any effect of tension could be observed when the circuit was completed; but the instant this was effected, the leaves of the electroscope as instantly collapsed, nor could I detect, either by the aid of the condenser or otherwise, the slightest trace of tension; it however immediately reappeared when the circuit was again broken*."

Thus, it is abundantly proved that as soon as the positive and negative poles of a voltaic series are brought in communication with each other, they comport themselves exactly as the positive and negative poles or conductors do of a common electrical machine; all symptoms of electricity cease. It is at this moment, however, and not until now, that the connecting wire of the voltaic series becomes magnetic. Is there not in this fact something repugnant to the idea that electricity is the agent? The magnetic properties appear when all electricity is neutralized and extinguished; and the moment that electricity is made to reappear by disconnecting the poles, the magnetism ceases. Professor Faraday himself, when treating of a different subject, expressly admits the neutralizing effects of the two electricities. Speaking of voltaic action, he says " it produces a current in which the opposite forces are so equal as to neutralize each other." What can neutralization mean if it be not that the properties of each are for the time suspended, and can no longer act.

To admit that the two states of electricity, after having neutralized and virtually annihilated each other's properties, should nevertheless at that moment be more active than ever in calling into operation an energetic power of a totally different nature, is contrary to every agency of electricity of which we have any real knowledge. By the neutralization of the positive and negative states of electricity, the natural condition of equilibrium is produced; the electricity is then quiescent as it was previously to the excitation that rendered it active; it is, in short, in the insensible state of the element as it exists throughout all nature. If in that state electricity be competent to excite magnetism, it must be admitted by the defenders of this hypothesis, that all bodies in nature are magnets, and even magnets of great power, a position which carries its own refutation.

This objection applies to the opinion of those who maintain that electricity considered as a simple element is the cause of, or is identical with, or excites magnetism; but not to the view which I suggested in the beginning of this essay of the compound nature of the electric fluid, one of its constituent elements being magnetism.

* Philosophical Magazine, 1844, p. 290.

It is no doubt true that frictional electricity has the power of communicating, reversing and destroying magnetic polarity; but it never does so while traversing a conductor in the silent quiescent way which voltaic electricity is known to do.   To produce magnetic polarity, it must be in the state of high tension, and the circumstances must be otherwise peculiar.   In this state it acts with much dynamic violence, and will communicate magnetism, as hammering, filing, and other mechanical causes are known to do.   Of the peculiar condition of electricity in the state of flash, and how it differs from a current traversing a conductor, we know nothing further than that there is a great difference.

The boldest of all the hypotheses of magnetism and the most ingeniously supported is that of Ampère.   This philosopher denies the existence of a magnetic fluid, or of a magnetic agent called into action by electricity; but affirms the absolute identity of both powers, an opinion first advanced by Beccaria and supported by Azais.   Notwithstanding the address, ingenuity, and resources of invention with which M. Ampère has constructed and applied his hypothesis, it does not seem to have made much advance in public opinion in the British Isles.   This doctrine scarcely comes within the province of my essay, as it does not indicate magnetism as a property common to frictional and voltaic electricity, but as identical with both.   Mr. Sturgeon's arguments* appear to me sufficient to invalidate the assumed identity, yet it may not be superfluous to describe two experiments which I made on this subject; especially as they both refer to the question whether electricity is magnetism, or whether electricity produces magnetic effects.   Their results were of course foreseen; I made them merely to permit me to use them as arguments.

A cylindrical rod of soft iron twelve inches long was wound round in the usual manner, from end to end, with copper wire covered with sewing silk.   This was supported in the middle horizontally by an upright glass pillow set in a wooden stand.   The ends of the copper wire were connected with a Smee's battery consisting of four triads of silver and zinc plates, acted on by very dilute sulphuric acid.   To the smooth, flat ends of the cylindrical rod were adapted two pendent flat iron armatures, each having a pair of gilt pith-balls attached by means of gilt strings.   In this state of things, the cylindrical iron rod being converted into a temporary magnet, its ends would hold the pendent armatures attracted; and the attractive force would, according to the hypothesis of Ampère, be the difference of electrical state between the ends of the cylindrical rod and the armatures.   Application of a

* Proceedings and Transactions of the London Electrical Society, 1838.

powerful permanent magnet over either end of the cylindrical rod, the permanent and temporary magnets thus presenting contrary poles to each other, ought by the neutralization of the opposite states of electricity to cause the armature of that end to fall off. On making the trial with a powerful horseshoe magnet the armature of that end, as might be expected, did fall.

So far the result corresponded with the hypothesis; but if electricity be really the agent, the transmission of a stream of common electricity from the prime conductor of a large and powerful cylinder machine should act like the permanent magnet in detaching either of the pendent armatures from the ends of the temporary magnet. On throwing a torrent of sparks, which to the eye appeared a continuous stream of fire from a cylinder capable at the moment of affording twelve-inch sparks, on the temporary magnet, the pith-balls, hitherto of course unaffected, diverged to a maximum, but the pendent armatures remained in their places.

It will be said by the supporters of the hypothesis in question, that the quantity of voltaic electricity passing through the coated copper wire, coiled round the cylindrical iron rod, was so much greater than that furnished by the glass cylinder machine, that the electrical state of the poles of the temporary magnet and of the armatures was not destroyed, and hence the armatures were still attracted. If this be so, it must be inferred, that as the voltaic electricity of the pole of the temporary magnet was neutralized by the opposite electricity of the permanent magnet when approached, so ought it also to have been neutralized by the stream of opposite electricity thrown in by the conductor of the electrical machine. That the *quantity* of the frictional electricity was sufficient for such a neutralization, was abundantly proved by the maximum divergence of the pith-balls with the same electricity as that of the prime conductor which supplied it. Why then were not the laws of electricity obeyed? Why did not the armatures fall off as well as when the permanent magnet was applied, if the agent in both cases were electricity?

Doubts founded on the foregoing objections, although they may not have been expressed, seem to have influenced the language of those who have alluded to the reaction of magnetism and electricity; and we discover remarkable reserve amongst writers and experimenters on this subject, arising no doubt from appreciation of the difficulty. Dr. Roget, in his excellent article on Electro-magnetism[*], thus expresses himself:—" At all events we know that two currents of electricity in motion, *impress* by their mutual action, a force differing very essentially from those commonly considered electric, and which affects the magnetic

* *Encyclop. Metropolit.*, par. 53.

needle." M. Colladon says that the two electricities unite and form a current which "produces" magnetism. Professor Faraday says that electricity and magnetism are "essentially associated." From none of these do we learn the nature of the connexion of the two agents, or the manner in which they reproduce each other. To have entered on this question would at once have led to the disclosure that neutralized electric states or fluids cannot exert any known agency.

A word has of late years come into common use, which, while it explains nothing, conceals the solecism contained in the notion of neutralized electricities retaining their respective energies. This word, "the current," has the effect of keeping out of view the counter-current, which is the grand difficulty, because it must antagonize and destroy the current. This modern current cannot have been derived from the old well-ascertained positive and negative currents of frictional electricity ; for these can be seen, felt and understood. But the new current consists of both ; and instead of being rendered powerless, as it was formerly the nature of oppositely electrical currents to be when commingled, it is only in this state of combination that the positive and negative electricities are said to be capable of exerting peculiar powers. The current seems to have been modified to meet the exigences of recently discovered phænomena ; but in its new acceptation, it no longer harmonizes with those from which our knowledge of the true current was derived.

The current being now used in the explanation of every voltaic fact, and its meaning not well-defined, it is important to discover what is really intended to be conveyed by the term. Professor Faraday says " it is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are everywhere in it. Any one part of the current, may, as respects the presence of the two forces there, be considered as precisely the same with any other part. It appears to me to be as impossible to assume a current of negative force alone, or of two at once with any predominance of one over the other, as to give an absolute charge to matter*." He explains that "a current is produced both by excitement and discharge." "Excitement may occur in many ways, as by friction, chemical action," &c.† We are therefore to understand that these observations are applied to the streams of electricity which pass from the conductors of an electrical machine, as well as to the currents from the poles of a voltaic series. The currents being in both cases, as is stated, always the same, and in every part the same, and the two forces everywhere present in it, it is to be inquired what is the nature of the stream

* Researches, p. 518.   † Ibid. p. 515.

of electricity which passes in sparks from either conductor of an electric machine, when any conducting substance is approached; are both streams mixtures of positive and negative electricity in equal ratio? if so, why do these torrents of fire appear? why do they not neutralize each other while *on* the conductor, and fall into inert equilibrium, instead of flashing through the air? why does a body charged with one kind of electricity attract a body charged with the other, and repel a body charged with the same? and lastly, if the composition of the positive and negative currents be the same, what is the difference between positive and negative electricity, and why have they different properties? That they have different properties, in respect of the class of phænomena under consideration, appears from the researches of Faraday himself; for in his decompositions of salts by frictional electricity, the results are described to be the same with regard to the distribution of the separated elements as would have been produced by the voltaic apparatus; and he concludes his observations on this subject by declaring that there cannot be a doubt " that voltaic and common electricity have powers of chemical decomposition alike in their nature, and governed by the same law of arrangement*."

The difference between the positive and negative conditions of the current is thus represented by Professor Faraday: he says the current is neither a compounded nor complicated influence, but " an axis of power having contrary forces exactly equal in amount, in contrary directions†." But here no real distinction appears to be established between positive and negative electrical currents; for in every part‡ of both the two electrical forces are present " in equal amount," " travelling §" in opposite directions; there is therefore no difference. As to the affirmed difference of direction in which the currents are said to travel, it is not easy to understand how that can give origin to the great difference of properties manifested by the positive and negative poles of a voltaic series. An absolute direction of the currents, in contradistinction to a relative one, can have no effect; for if it had, reversing the position of a voltaic trough ought to reverse its poles. It must then be the relative direction of the currents with regard to each other that is meant. What the influence of even relative direction may be is not very intelligible; for current ⇄ seems not to differ from current ⇄ when both constituent forces are exactly of the same constitution, as is affirmed.

* Researches, par. 331.   † Ibid. par. 517.
‡ Ibid. par. 1642.   § Ibid. par. 1635.

If, then, the current consist of positive and negative electricity, equal to each other in amount and force, how does the state resulting from this commixture differ from that of the natural equilibrium of electricity, which, in a well-known state of quiescence, pervades all nature and manifests no properties? How is this state of quiescence compatible with the idea of a current? A current of what?—is it of insensible electricity without manifest properties, neither positive nor negative? If so, how is its presence recognised? The existence of a thing is known by its properties; if there be no properties, the thing contemplated exists only in imagination.

The nature of the current is explained by Faraday as follows:— "By current I mean any thing progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or speaking more generally progressive forces*." Supposing the current for the present to consist in vibrations, it seems very difficult to associate in the mind the two conditions assumed to be compatible, viz. vibrations of solid, rigid matter†, along with progressive forces. Were electricity assumed to be a fluid, a current or progressive force is conceivable, or two such; but if there be no fluid, the idea of a force which progresses or moves forward is difficult to comprehend; and in that case, what is it that constitutes the current? what flows in it? can vibrations of the particles of an electrified substance be *permanently* progressive during the whole period in which the electric state is maintained? The vibrations of all the constituent particles of an electrified mass being once established, they may continue; but can they be progressive, especially in opposite directions? For my own part I do not conceive the meaning of the expression " progression of permanent vibrations of fixed particles," for the current resolves itself into this: and is the *idea* of a current included in the *expression* of it, when it is at the same time declared that probably nothing flows? I by no means intend to insinuate any doubts concerning the assumption of vibrations amongst the constituent particles of rigid matter: the theory of Boscovich relative to the constitution of solids, provides for the possibility of such. Professor Faraday, without admitting or rejecting the doctrine of two fluids, or one fluid, or none, or vibrations of the subjective matter, endeavours to harmonize the idea of a current of progressive forces with all these contingent opinions. But if we abstract from all consideration of fluids and vibrations, it strikes me that " progressive forces"

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* Researches, par. 283.

† This is the sense in which Faraday intends " vibrations" to be taken. Ibid. 1667.

are words which do not represent any change, and cannot serve in the explanation of phænomena.

It is also to be observed that the theory in question assigns no *cause* for the assumed progression of forces, or for the current in a metallic conductor. Admitting that the particles of the subjective matter are thrown into a state of electric vibration ; admitting also that such a state can be thrown into a progressive current, however difficult that may be to comprehend, one sees no reason why the particles should not quietly vibrate, each in its respective place, no cause being assigned for the abnormal or forced condition of a progression. Besides, the electric vibrations are in this theory turned to no account ; they explain nothing ; they are not represented as producing any results ; there is no evidence of their existence ; they are gratuitous assumptions, which may be admitted or denied without benefit or detriment to the theory.

In what then does this current consist ? what is it that produces such remarkable phænomena ? wherein is the use of assuming its existence ? and what advantage do we derive from the employment of a term to which no definite object or meaning is attached ? A current of electricity in which no electricity is affirmed to flow, which is said to be independent of one or two fluids or of vibrations of matter or æther, the nature of which is admitted to be utterly unknown, appears to be a creation of the mind which has no archetype in nature.

I have thus freely expressed my opinions relative to the current, fearing that the old legitimate sense has been lost sight of ; that many have understood it to mean something more than is warranted by any *proved* properties ; and that the universally admitted identity of the agent in electric and voltaic phænomena has emboldened philosophers to attribute qualities to the former which belong only to the latter. On the whole, I conceive that the current, in its modern acceptation, instead of explaining voltaic phænomena, is calculated to mislead, and that it is of no avail in obviating the difficulties which beset the alleged simultaneous operation of the two states of electricity after commixture ; which states, instead of being at that moment in their condition of greatest energy, should be destitute of all sensible properties.

[To be continued.]

VI.   *On the Composition of Wootz, or Indian Steel.*
By T. H. HENRY, *Esq.*, *F.R.S.**

THE high degree of estimation in which wootz has been held in this country appears to rest more upon the supposition that the celebrated scimitars of Damascus were made from this variety of steel, than on any results obtained with it here; for notwithstanding the late Mr. Stodart, an eminent authority, was of opinion that wootz was superior for many purposes to any steel commonly used in this country, the attempts to bring it into use have not been successful, owing, it is said, to the difficulty of working it.

Under these circumstances, it appeared to me desirable to ascertain as accurately as possible the chemical composition of this steel, with the hope of throwing some light upon the causes of its peculiar physical properties.

An examination of wootz was made by Dr. Faraday in the year 1819†. The amount of carbon was not determined by him, the only substances eliminated were silica and alumina; and he obtained in two analyses 0·0128 and 0·0693 per cent. of aluminium.

From these results Messrs. Faraday and Stodart drew the conclusion, that the peculiar excellence of wootz depended chiefly on the small quantity of aluminium combined or alloyed with the steel‡, and this opinion appeared to be strongly supported by ingenious synthetical experiments.

On the other hand, Karsten could only detect dubious traces of aluminium in wootz; and Elsner§ attributed the improvement in the quality of the steel produced in Messrs. Faraday and Stodart's experiments, not to the small quantity of the foreign metals, aluminium, silver, platinum, &c., alloyed with them, but entirely to the operation of remelting, and this seems to be the practical conclusion come to at Sheffield at the present day. The fact, however, of the perfectly damasked surface obtained in the alloys of Messrs. Faraday and Stodart so closely resembling that of wootz, seems to militate against the conclusions of Elsner.

M. Breant attributes the damask of the Eastern blades to the crystallization of two distinct compounds of iron and carbon, and draws a distinction between the oriental damask and that produced by alloys of steel. This opinion is confirmed by the experiments of M. Anocoff, a Russian engineer, published in the *Annuaire du Journal des Mines de Russie*, a few years back. He

pretends to have produced blades so nearly emulating those of Damascus, as to allow of their being bent at a right angle, and capable of dividing a film of gauze floating in the atmosphere*.

I obtained from my friend Mr. Trenham Reeks of the Government School of Mines, two samples of wootz, furnished to him by Mr. Lewis Humbert of the military department of the India House; one was in the form of a cake, such as would be produced by allowing the melted steel to cool in the crucible; the other was forged into a small bar, about 4 inches long and 1 inch square, and weighed 4760 grs., or rather more than 11 oz. These are the forms in which it is imported into this country. I preferred operating on the bar, for in steel in this form small particles of slag are often so intimately mixed with the metal as to defy separation; and it is possible, as all the alumina found by Dr. Faraday in wootz was in an insoluble form, that it might have existed as silicate of alumina.

The specific gravity of this specimen was 7·727 at 62° F.

Analysis.

To determine the total amount of carbon, the steel in its soft state was reduced by means of good files to such a minute state of division that it passed through copper-wire gauze containing 8100 holes to the square inch. The files employed were those used for polishing, being "single cut;" they are not so hard as the "cross cut" files, and consist of sharp edges instead of points, and consequently are not so liable to abrasion; when used with care there is no danger of any particles of the file mixing with the steel operated on.

It was then burnt with oxide of copper only, as Kudernatsch recommends. I believe the most accurate results are obtained by this method. I could not find any iron remaining in the metallic state after combustion; it appeared all in the state of magnetic oxide.

I obtained in two experiments, operating in one case on 60 grs. passed through the gauze of 8100 holes to the square inch, and on 50 grs. passed through gauze of 14,400 holes to the inch,—

I.	II.
1·645 per cent.	1·652 per cent.

The amount of uncombined carbon in the form of graphite, was determined in the usual manner, by dissolving in hydrochloric acid, in a platinum vessel, evaporating to dryness without filtration, separating the silica from the graphite by caustic pot-

* A specimen of his damask steel is to be seen in the Museum of the Government School of Mines in Jermyn Street.

ash, and igniting the residue. I obtained thus 0·312 per cent. of graphite; the solution in caustic potash was acidulated by hydrochloric acid and evaporated to dryness; the silica remaining was equivalent to 0·045 per cent. of silicium; hydrosulphuric acid afterwards precipitated 0·037 per cent. of arsenic; not a trace of alumina could be obtained from this solution by hydrosulphate of ammonia.

The solution of the iron in hydrochloric acid was treated by carbonate of baryta, the precipitate redissolved, the baryta removed by sulphuric acid, the iron precipitated by ammonia dissolved in hydrochloric acid and boiled with pure caustic potash: no alumina was extracted.

The solution of the steel from which the iron had been separated was, after separating the baryta, evaporated to dryness in a platinum dish; the residue did not yield a trace of manganese, magnesia, lime, cobalt or nickel.

To determine the amount of sulphur to control the determination of the silicium and arsenic, and also to give me another opportunity of searching for aluminium, I treated 50 grs. of the steel in the above minute state of division with pure nitrate of soda, mixed with a little carbonate of soda, at a red heat, in a crucible of pure gold*; the action was easily controled, and the oxidation was complete; the mass was treated with warm water, the solution acidulated with hydrochloric acid and evaporated to dryness. I obtained thus 0·042 per cent. of silicium, and afterwards by chloride of barium 0·181 per cent. of sulphur, and in another experiment 0·170 per cent. of sulphur; the excess of baryta was removed by sulphuric acid, and the arsenic precipitated by hydrosulphuric acid gave 0·036 per cent.; no alumina could be found in this solution, and only faint traces of phosphoric acid. I have not been able to determine the amount of sulphur in steel or cast iron nearly so accurately, by the methods of Berzelius or Karsten, as by the above process.

I was not able to detect alumina in the residue left on dissolving 500 grs. of this steel in acid, nor on decomposing 117·87 grs. by means of a cake of fused chloride of silver. The residue left on the cake of silver weighed 3·81 grs., and lost 2·213 on ignition, which required to be continued some time before the odour of arsenic disappeared; if we deduct the amount of sulphur and arsenic from the loss, this will give 1·660 for the total amount of carbon; but I do not consider this method so accurate as that of

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* I have found it better to alloy the gold with 5 per cent. of platinum, which increases the hardness and renders it one of the most useful instruments which can be employed in delicate analysis; gold alloyed with 10 per cent. of platinum does not appear to be acted on by nitrates, and is very hard.

combustion with oxide of copper, although it may appear to confirm the result in this case.

The composition of this steel then will be :—

	I.	II.
Carbon combined	1·333	1·340
Carbon uncombined	·312	·312
Silicium	0·045	0·042
Sulphur	0·181	0·170
Arsenic	0·037	0·036
Iron	98·092	98·100
	100·000	100·000

VII. *On Copper Smelting.* By JAMES NAPIER, *F.C.S.*[*]

AS no description of copper smelting in all its details has been published in this country, it may not be uninteresting, or without use to the readers of this Journal, were such a descriptive account given of this extensive and useful branch of manufacture.

Copper is one of the seven metals which were known to the ancients; and it seems to have been known from the very earliest times, and used extensively for instruments of war and for domestic purposes. The oldest remnants of the metallic art are composed of copper and tin. The great beauty which many of the ores of copper have, and the ease with which such ores are reduced to the metallic state, may have brought it early into notice and use; and that the metal was obtained from the ore by fusion or smelting, is indicated in one of the earliest writings extant. Job says,—

"Copper is molten out of the stone."

Copper is occasionally found in nature in the metallic state in considerable quantities, both in this country and abroad. The great masses found at Lake Superior form an eminent example of this; but the chief source of copper is its ores, which constitute, in combination with other substances, such as oxygen, sulphur, carbonic acid, &c., an extensive variety of minerals of distinct forms and character, most of which have been the subject of careful chemical investigation. The following table of the names and composition of some of these minerals will give some idea of their variety.

Name of mineral.

	Eukairite.	Tennantite.	Gray copper ore.	Bournonite.	Copper pyrites.	Variegated copper ore.	Hexamuriate of copper.	Silicocarbonate of copper.	Hydrocarbonate of copper.	Octahedral arseniate of copper.	Amianthiform diarseniate of copper.	Acicular oliven ore.	Prismatic oliven ore.	Diarseniate of copper.	Copper Schaum.	Hydrous subphosesquiarseniate of copper.	Bisilicate of copper.	Hydrous sesquisilicate of copper.	Hydrous subphosesqui-phosphate of copper.	Hydrous diphosphate of copper.	Tetrasulphate of copper.	Sulphate of copper.	Hydrous dicarbonate of copper.	Anhydrous dicarbonate of copper.	Disulphuret of copper.
Copper	23·05	45·32	40·60	12·80	32·05	63·34	76·6	50	70	30·10	54·98	56·65	56·43	47·85	43·88	59·4	35·1	55	62·84	63·9	45·9		70·25	60·75	78·5
Oxide of copper							10·6														30·5	32			
Sulphur		28·74	26·83	17	35·01	24·69																			18·5
Iron		9·26	4·66	1·20	32·04	11·8																			2·25
Silica		5·0	0·41			0·166		26									35·4	33						2·0	0·75
Sesquioxide of iron	26 (Selenium)																1·0								
Carbonic acid							(Muriatic acid)	7	24														19·62	19·50	
Water	8·90 (foreign earthy matter)						12·8	17	6	26·69	4·41	3·55	3·5	18·8	17·46	5·01	28·5	12	15·45	7·4	12·1	36	10·12	16·70	
Sulphuric acid																					11·5	32			
Phosphoric acid																1·77			21·68	28·7					
Alumina													3·36												
Arsenic acid		11·84	10·19							43·39	40·61	39·8	36·71	33·02	25·01	33·78									
Carbonate of lime															13·65										
Antimony			12·46	24·35																					
Silver	38·93		0·60																						
Lead			3	41·12																					
Zinc			·69																						

The above table is exceedingly valuable to the chemist and mineralogist, from its exhibiting the various combinations of copper with other matters, and illustrating the chemical action going on in the bowels of the earth; still tables of this sort are of little use to the mere practical man. The minerals here named and analysed have been carefully selected and freed from everything mechanically mixed; many of them have been found only in minute quantities, and therefore can form no distinct division or department in the practical operation of smelting; and whether the peculiar ingredients that give the distinctive character to the mineral be deleterious or otherwise, their separation mechanically is altogether impracticable. The smelter may be said to have only two classes of ores, those that contain sulphur, and those that contain no sulphur; however, from the knowledge that some ores contain matters which make them more fusible than others, and also matters that combine with the copper, making it of inferior quality, a more extensive division is observed, such as *sulphurous ore* (copper pyrites), *mundicy ores* (ores containing *mundic* or iron pyrites), *gray ore*, *tiny ore* (containing tin), &c. Many of these distinguishing characters depend more upon the foreign matters mixed mechanically with the copper mineral than forming a chemical constituent of it; hence the smelter has a far more extensive class of substances to deal with in his practice than is named in the table of copper minerals given above.

Metallic minerals or ores are found filling up cracks or fissures of the rocks forming the crust of the earth, and are termed veins. The minerals composing a vein are generally of a great variety of kinds, containing often copper, tin, antimony, bismuth, iron, nickel, cobalt, arsenic, manganese, silver, &c., besides what are termed the earthy minerals or matrix, such as quartz, lime, slate, &c. In mining, the contents of the vein are taken out, so far as it contains any of the metal or metals sought after; so that what is technically termed a copper ore is often a mixture of everything that the vein contains. And when it is mentioned that the average per cent. of copper in the ores raised in this country is 8, it will be seen that the matters mixed with the copper mineral forming the ore must act a prominent part in the smelting operation; and the action and reaction of these substances, when passing through these operations, must be attended to by the practical smelter.

The principal substance forming the matrix in copper ores is quartz. The relation of this to the other ingredients is the first thing to be considered by the smelter, as it is the first thing to be got quit of; and for this purpose the relation of the four substances, copper, sulphur, iron, and quartz, is the leading feature. To give some idea of the character of the copper ores in this country in respect to these matters, we append the following table of ores from various mines of Cornwall and Devonshire, tested for their smelting quality, which we shall occasionally have to refer to.

Name of mine.	Copper.	Iron.	Sulphur.	Silica.
Poldice	8·7	32·2	10·1	38·6
Tincroft	7·5	33·0	13·5	30·4
Great St. George *Fluccan*	12·5	8	15	63·2
Wellington Mines	10	22·5	17·3	45·1
Great Mitchell Consols ...	4·1	34	26	19·8
Grambler....................	11·2	19·4	16	40·9
Wheal Sisters	7·5	26	17	40·1
East Pool	7·5	21·3	10·4	46·5
Wheal Maria	9·3	36·8	22·1	12·1
Wheal Bucketts	7·5	18·2	8·6	54·3
Tincroft *Fluccan*............	9·5	21·8	7·2	46·7
Tremane	19·1	16·2	19·1	39·1
Condurrow	8·1	37·1	15·3	21·7
North Pool	10·6	27·8	19·3	31·1
Wheal Jane.................	7·8	25·1	19·6	36·5
South W. Basset............	8·4	10·5	13·3	62·3
Wheal Williams	10·9	34·1	14·9	25
United Mines	7·7	20·3	12·6	46·8
W. Rodney	4·1	24·5	19·6	43·5
Gwinear Consols............	11·12	22·3	11·9	40
Consols	9·4	22	16·5	43·4
Wheal Friendship, Devon	15	25·5	20·7	24·5
Wheal Henry	10·1	22	15·4	38·4
South Wheal Turbine ...	8·7	11	8·4	65·8
Treleigh Consols............	12·8	22·3	15·0	38·4
Tingtang Consols	5·6	12	5·9	66·6
Wheal Ellen	8·7	16·9	13	51·5
Treviskey.....................	8·7	15·6	13·4	51·8
East Crinnis	12·5	16·8	16·9	41·5
Par Consols.................	10·8	28·9	15·5	43
Trethellan	4·6	26·8	11·5	37·5
Great St. George	6·3	20·7	16	48·2
Wheal Comfort	3·6	22·8	21·7	38·2
Wheal Harriett	7·5	28·7	10·1	26·7
Trymainhayle	5·6	8·4	13·6	50
North Roskear	7·5	22·4	14·7	43
Wheal Agar..................	6·6	17·8	12·1	53·5
East Crofty	6·9	31	12·5	33·7
South Caradon	9·4	31·8	14·1	31·7
Fowey Consols	14·1	31·8	17·6	23·1
Wheal Seton	8·4	24·1	14·9	40·6
Tresavean	5·6	17·8	12·6	48·3
South Wheal Francis	8·1	3·0	6·1	76·0
Wheal Jewel	17·8	3·5	5·6	76·5
West Caradon	11·2	27·2	14·8	30·7
Carn Brea Yellow Mine...	11·3	17·5	14·2	47·5
Wheal Tremayne	12·2	16·1	9·1	51·5
South W. Tolgus	6·6	23·5	14·4	42·2
W. Andrew Nangiles	11·3	23·1	9·3	42
Carn Brea Fluccan	10·6	13·3	7·8	60·1
Consols Fluccan	5·6	18·2	11·6	54·4
Wheal Clifford.............	9·6	19·5	7	53·7
North Basset	10	16·5	24	41·1
W. Prosper	4·4	17·5	8·9	53·5
W. Mary Consols	10	28·7	19·5	29·5
Tresavile Barrier	8·7	15·1	10·3	57·2
Bedford United Mines ...	10	32·8	17·6	21·7
Dolcoath.....................	10	32	15·3	26·6
Great Work.................	14·1	18·9	14	43·5
W. Maiden	5·6	17·1	11·8	54·0
Camborne Vean	8·7	34·8	13·2	28·5

The mines here named may contain most of the minerals enu-
merated in the first table in small quantity; but when the whole

contents of the vein are crushed and mixed, they assume a sameness of character. Many of them contain lime, alumina, and some other earths; most of them contain antimony; some of them tin, arsenic, manganese, &c., all of which play an important part in the after operations. There are also a great many other ores not referred to in the above table, all of which, when mixed, as they must be in smelting, exhibit the same general character.

Besides these ores of Cornwall and Devonshire, great quantities come from Ireland and different parts of Wales: and vast quantities are also imported from other countries, all of which coming into the smelting-house to be worked up together, must be taken into account in the general description; and as many of the foreign ores are very rich in copper, more so than it is found œconomical to work by the ordinary process of smelting, the smelter is thus not only enabled, but somewhat necessitated, to buy poor ores to mix with and dilute these rich ones. Thus very poor ores in this country, which might otherwise have been unsaleable, are required; so that the importation of rich ores is not, as has been often asserted, destructive to our poor mines.

Name of mines and locality.	Copper.	Iron.	Sulphur.	Silica.
Irish Ores.—Knockmahon	8·3	19·9	14·2	57·6
... .	11·5	16·6	16·7	55·4
Casheen	17·5	12·3	10·2	58·7
Berehaven	10·2	10·8	14·8	63·9
Holyford	8·2	36·3	40·8	14·3
Anglesea.—Parys Mines	22·0	27·2	28·0	20·8
Cuba.—Cobre	23·2	30·4	20·8	19·6
... .	13·5	34·1	21·3	31·1
... .	18·2	33·8	28·8	19·2
... .	14·4	28·0	24·0	34·0
Cobre Dust	12·0	24·3	18·1	44·2
Cobre Rough Ore	19·3	18·3	20·7	40·1
Cuba Dust	14·9	26·1	27·1	32·0
Cuba Rough Ore	20·5	28·4	30·2	21·0
Chili.—Chaco Mines	58·3	14·9	23·7	3·0
Copiapo	26·3	22·4	19·9	19·3
... .	30·0	21·4	29·3	16·8
... .	24·0	23·5	20·4	14·0
Regulus	52·2	30·3	14·3	3·2
... .	59·6	15·1	19·1	4·6
New Zealand..............................	12·7	33·9	35·1	18·3
... .	11·0	20·3	15·5	53·3
Australian Ores.—Burra Burra	39·4	12·4	......	20·0
... .	37·4	6·0	......	19·0
... .	44·0	4·2	......	20·2
... .	37·0	4·7	......	38·6
... .	22·0	33·7	......	23·2
... .	16·0	5·7	......	50·0
... .	18·1	3·0	......	54·0
Princess Royal ...	32·0	19·5	......	22·0
... ...	22·0	17·8	......	37·8
Kinmantoo	17·6	18·4	7·7	38·2
Kapunda	16·0	3·0	......	60·0
... .	25·4	14·8	6·5	30·1
Paringa	18·8	16·3	......	27·8
... .	24·5	21·0	3·16	28·0
Molong	28·6	4·0	19·3	49·0

The preceding table of the same constituents in ores from other mines brought into market, will bring out the distinguishing features of the variety of ores the smelter has to operate upon.

By comparing this table with the former, we find a far greater variety of ores, and, taking the mines separately, a far more distinctive character, as will be observed by comparing the Cobre, Chili, and Australian together. At the same time there is a decided sameness about the character of the ores from the same locality, as will be seen in the Irish, the Cobre, and Burra Burra. As the latter mines are probably the wonder of the present age in regard to their richness and abundance, and as many of the ores found in them are too poor to be brought to the smelting-works of this country, I here subjoin a table of analyses from this mine alone, furnished me by my friend Mr. A. D. Thomas, chemist at the Burra Burra smelting-works.

Burra Burra Ores.

	Oxide of copper.	Protoxide of iron.	Carbonic acid.	Water.	Silica and alumina.	Total.
1	22·50	2·90	10·94	2·00	61·70	100·04
2	18·75	40·00	17·44	1·75	22·00	99·94
3	18·75	31·40	12·60	1·50	37·00	100·75
4	25·37	28·35	16·15	1·00	28·50	99·37
5	23·75	8·55	11·05	1·00	55·55	99·90
6	27·00	35·00	12·20	5·00	20·50	99·70
7	19·75	25·00	9·40	4·20	41·50	99·85
8	25·00	19·35	12·50	3·20	39·50	99·55
9	25·25	34·00	10·50	3·70	26·50	99·95
10	26·25	20·25	11·20	4·00	38·00	99·70
11	25·62	34·68	13·40	1·80	24·50	100·00
12	21·31	38·25	8·94	2·50	29·00	100·00
13	20·60	48·00	12·00	2·40	17·00	100·00
14	18·75	36·85	8·00	3·40	33·00	100·00
15	19·00	39·50	11·00	1·40	32·00	102·90
16	26·50	43·00	11·00	2·00	17·50	100·00
17	36·85	9·00	13·40	1·10	40·00	100·37
18	20·00	35·00	12·50	1·50	31·00	100·00
19	34·00	7·00	15·00	2·00	42·00	100·00
20	18·12	37·80	13·88	1·71	28·50	100·00
21	18·75	28·50	12·70	2·50	37·50	99·95
22	35·62	2·00	17·00	1·90	43·50	100·02
23	18·75	32·85	12·90	2·50	33·00	100·00
24	18·75	34·15	11·10	1·00	35·00	100·00
25	18·75	32·85	16·10	1·30	31·00	100·00
26	26·37	35·55	12·60	1·50	31·00	100·00
27	21·25	42·75	16·00	2·00	18·00	100·00
28	23·41	33·75	16·60	2·30	22·50	100·56
29	21·90	40·50	17·40	1·70	18·12	100·00
30	37·62	10·35	17·83	1·70	32·50	100·00
31	34·40	7·20	17·50	2·00	38·50	99·60
32	29·30	12·15	12·10	2·40	44·00	99·95
33	32·25	20·25	17·10	2·30	28·00	99·90
34	30·60	10·00	13·80	2·10	43·50	100·00
35	34·40	6·50	15·10	2·00	42·00	100·00

It may be remarked, that the ores given in the above table

are not selected specimens, but are taken from and after the mass is crushed and ready for the furnace; and they are mostly all poorer in copper than those from the same mines that are imported into this country.

The two grand divisions of ores in the smelting-house are those with and without sulphur, constituting oxides, carbonates and sulphurets, a few of which we will briefly describe. There are two oxides of copper, the black and the red. The former, as its name denotes, is a black crystalline mineral inclining to brown and blue; it often occurs as a friable mass, and forms a sort of coating over the surface of other ores, such as the sulphurets when they have been subjected to exposure to air and water. Black oxide is not found in great quantities in a pure state, but in combination with iron it is abundant in some of the Australian mines. When pure, the composition is—

Copper	.	. .	79·82
Oxygen	.	. .	20·18
			100·00

It is abundant in the Burra Burra mines. The average of a good many analyses of selected specimens may be stated to be—

Copper	.	. .	72·6
Oxygen	.	. .	18·1
Protoxide of iron	.		4·3
Silica	.	. . .	4·5
			99·5

Red oxide of copper differs from the black oxide by containing less oxygen; it is more abundant as an ore than the black, has a reddish-brown colour, often approaching to rich red, especially when crushed. When native copper occurs, it is generally surrounded by red oxide; it is associated in the Australian mines both with the black oxide and carbonate. When pure its composition is—

Copper	.	. .	88·8
Oxygen	.	. .	11·2
			100·0

This oxide has occasionally been termed *tile ore*, said to be from its colour; but we think this a corruption. There are many poor ores which have the same colour, in mass, from the matrix being a rich red clay, *red fluccan*: the copper made from some of these ores being inferior, is termed *tile copper*, hence probably the error. We have specimens of red oxide from Cornwall forming small veins through other qualities of ore, which contain upwards of 82 per cent. of copper, with a little silica and iron as impurity. A massive specimen from Burra

Burra forming a vein between black oxide, gave—

> Red oxide . . . . 93·5
> Silica . . . . 5·2
> Oxide of iron . . . 1·1
> ———
> 99·8

A specimen from Chili diffused through carbonate of lime, gave—

> Red oxide . . . . 48·3
> Carbonate of lime . 48·5
> Oxide of iron . . . 1·2
> Silica . . . . . 1·3
> ———
> 99·3

The colour of this mineral is a rich red: the lime is mechanically mixed, and can be seen by the microscope.

There is another red ore which we have seen in considerable quantity, resembling much the red oxide in appearance, only wanting in lustre and specific gravity; it generally occurs massive, having occasionally fine red veins through it. A specimen from Cornwall gave—

> Oxide of copper . . . 24·8
> Peroxide of iron . . 51·5
> Silica . . . . . . 19·0
> Water . . . . . . 4·1
> ———
> 99·4

A specimen from Chili, of a similar colour and appearance, gave—

> Oxide of copper . . 35·5
> Peroxide of iron . . 30·2
> Silica . . . . . 18·0
> Carbonic acid . . . 7·2
> Water . . . . . 9·0
> ———
> 99·9

These two would rank as tile ore from the colour, but would produce excellent copper.

Carbonates of Copper are of two kinds, blue and green, and are very easily distinguished by their appearance; or they may be easily tested by effervescing when put into an acid. The blue carbonate is of a deep azure; when crystallized, the azure blue is permanent; but when massive, the colour is much paler, especially when dry, becoming very rich and deep when moistened. This ore may be easily distinguished from the blue vitreous ore from the colour being more intense; it is also softer when scratched with a knife.

Green Carbonate of Copper is easily distinguished by its rich grass-green colour; it has a considerable lustre, is harder than the blue carbonate, and the crystals are generally fibrous. A variety of this mineral, which is generally found in nodules formed of a series of concentric layers, is known as malachite, which, when cut and polished, is of great beauty, and is consequently used for ornamental purposes. The Russian mines were long famed for their malachite ore, but it is now found in great quantities in some of the Australian mines. The difference of composition of the blue and green carbonate of copper is, that the one contains more chemically combined water than the other, which is no doubt the cause of the difference in colour. The following is their composition when pure:—

	Blue.	Green.
Copper oxide	70	70·5
Carbonic acid	24	18·0
Water	6	11·5
	100	100·0

For the composition of the various carbonate ores, we refer to the table of Burra Burra ores.

Sulphuret of Copper.—This is a very abundant ore, and has a great many varieties; the colour is lead-gray; it is vitreous in appearance, and compact, often assuming a blue tint upon the surface; it is very heavy, and easily distinguished. As an ore, it is generally associated with iron and silica; it is an abundant ore in Chili. A great mass of this ore, which was exhibited in the Great Exhibition, gave—

Copper	55
Sulphur	25
Iron	14
Silica	6
	100

It is often found richer than this, but this analysis may be taken as the average composition.

Gray Copper Ore is named from its having a steel-gray colour. It is a very common ore of copper, and is the principal mineral found in some of the Cornish mines. This mineral is very variable in its composition, scarcely two localities giving the same. The following analyses will illustrate this variety:—

	Cornwall.	Devonshire.	Algeria.
Copper	15·5	12·5	20·3
Sulphur	23·7	15·6	14·2
Iron	41·7	15·0	4·6
Antimony	5·6	4·1	7·5
Arsenic	3·1	0·8	5·0
Zinc	...	...	1·1
Silica	8·5	51·6	47·3
	98·1	99·6	100·0

Copper Pyrites.—This is by far the most abundant ore of copper brought to the smelting-works. It is easily distinguished by its rich brass tint from iron pyrites, which is generally not so yellow; neither is it so hard, copper pyrites yielding easily to the knife. The two pyrites are often mixed together; and when so in a massive state, a trial of the hardness may give some idea of the quality of the ore. The composition of copper pyrites is a double sulphuret of copper and iron chemically combined. When pure, it is composed of—

Copper	34
Sulphur	34
Iron	32
	100

As an ore, it is almost invariably associated with iron pyrites. The quantity may be easily ascertained by an analysis being made for the quantity of copper, sulphur and iron. As an illustration, take what is termed *Cobre dust*, which is a mixture of iron and copper pyrites. A fair sample gave the following composition :—

Copper	14
Iron	26
Sulphur	26
Silica	34
	100

The copper, to form copper pyrites, will require—

Copper	14
Iron	13
Sulphur	14,

leaving $\left\{ \begin{array}{l} \text{Iron} \quad 13 \\ \text{Sulphur} \quad 12 \end{array} \right\}$ as iron pyrites, which is not far from being correct; the composition of that mineral being 28 iron and 32 sulphur; but in analyses of copper and iron pyrites mixed, the iron almost always prevails, probably from a portion being combined with the silica. We thus see, that by observing the chemical character of ores, much may be done, even in commercial samples, to distinguish the kind of mineral present; but we shall have occasion to refer to this subject more fully in future papers.

To return to the obtaining of the ores from the mines. When the ores are raised from the mines, they are broken up and dressed, separating by mechanical means, as far as possible, the earthy matters from the mineral; they are then crushed into small pieces, and collected into a heap preparatory for sampling for sale.

Each smelter has a resident assayer near the mines. All the ores to be put up for sale on a given day are announced; and two or three weeks previous, the different assayers, or others appointed as samplers along with the agents appointed by the sellers of the ore, proceed to the yard where the ores are, and take samples, which is done by taking a portion from every part of the heap, mixing them together, grinding the whole fine, and spreading it out upon the ground. It is then divided by lines drawn at right angles thus—

The two opposite portions, such as 1 and 3, are taken, the others thrown back into the heap. This is again repeated with the parts kept until the quantity remaining be sufficient to allow every sampler about 1 lb. weight. These samples are carefully examined by the assayers, each for his own employer. A few days before the sale takes place, the assayers meet, and a list of the lots taken, with the sellers' produce, according to assay, is annexed; each assayer gets a copy of this list, which he sends to his employer with his own private assays and remarks. The sale is effected by what is termed ticketing. On the day appointed for sale, the purchasers and sellers meet, when a neutral person is appointed to the chair, who is furnished with a list of the various lots for sale. He begins by reading over the number and particulars of the lot, when each purchaser writes upon a slip of paper the price per ton of ore he offers, and hands it to the chairman. These are read out and marked down, and the highest bidder is declared purchaser; should two or more bid the same, the ore is divided among them in equal parts. No second bid is allowed, nor any parcel of ore withdrawn. By this simple means, many thousand pounds' worth of ore are sold in a few minutes. The question has been put, whether this be a public sale. Certainly no stranger is allowed to bid. An intended purchaser must give a month's notice previously before being allowed to offer, to ascertain the responsibility of the party; but what common law would say is doubtful were a purchaser present with cash.
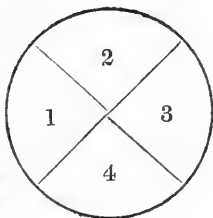
After the sale is over, an account is printed of the particulars of the sale, containing every offer; by which means, and comparing past accounts of sales, a very accurate approximation may be made of the stock of each class of ores which any of the purchasers have on hand; this does much to regulate the market, for a certain class of ores is sometimes of more value to a purchaser than at other times; and the object of the other buyers being to make the ore as dear to his neighbour as possible, the value of the ore is thus maintained. The following is a copy of a ticketing paper, which will illustrate our remarks:—

Copper Ores, sampled December 20, and sold at Swansea, January 11.

Mines.	21 cwt.	Produce.	1. English Copper Co.	2. Freeman and Co.	3. P. Grenfell and Sons.	4. Crown Copper Co.	5. Sims, Willyams, Nevill, Druce & Co.	6. Vivian and Sons.	7. Williams, Foster and Co.	8. Mines Royal Co.	9. J. Schneider and Co.	Excess.	Amount of each parcel.	Amount of each mine.
1. Cobre.—Pascoe Grenfell	80	23½	13 10	13 10 6	14 10 0	14 10 0	13 10 0	14 16	14 14	6 13 5	13 10	0 1	1184 0 6	
2. ...	77	22½	13 14	13 14 0	14 10 0	14 10 0	13 17 0	14 2	14 2	6 13 5	13 0	0 0	1087 12 6	
3. ...	72	23¼	13 15	13 10 0	14 10 0	14 10 0	13 15 0	14 15	14 14	6 13 5	14 1	0 0	1062 5 0	
4. ...	72	22¼	13 15	14 13 0	14 14 0	14 10 0	13 15 0	14 16	14 14	6 13 5	14 1	0 1	932 0 8	
5. ...	63	21½	13 6	13 2 0	14 6 0	14 0 0	13 5 0	14 0	14 2	6 14 0	13 18	0 3	781 8 8	
6. ...	59	21¼	14 10	14 2 0	14 10 0	14 18 0	14 0 0	14 18	13 2	6 13 0	13 13	0 2	784 14 6	
7. ...	53	23¼	13 10	13 3 0	14 16 0	14 10 0	13 0 0	14 11	15 1	6 13 0	13 18	0 2	798 19 6	
8. ...	52	21½	13 10	13 18 0	13 16 0	14 18 0	13 0 0	14 19	13 14	6 13 15	14 8	0 4	718 18 6	
9. ...	48	21¾	14 10	14 0 0	15 5 0	14 0 0	14 10 0	14 19	13 14	6 14 0	14 18	0 0	716 8 6	
10. Lady Prudhoe...	42	23¾	8 0	8 8 0	8 18 0	8 14 0	8 8 0	8 16	15 4	5 8 5	8 6	0 2	642 12 6	
11. ...	105	14½	8 0	9 5 0	8 2 0	8 14 0	8 15 0	8 9	8 17	5 8 5	8 7	0 1	937 2 6	
12. ...	104	14¼	8 0	8 10 0	9 0 0	8 13 0	8 18 0	8 0	8 16	5 8 5	8 1	0 2	954 4 0	
13. ...	101	14½	8 0	8 12 0	9 19 0	8 17 0	8 10 0	8 18	8 16	5 8 10	8 9	0 1	911 10 6	
14. ...	100	14½	8 0	8 10 0	8 15 0	8 16 0	8 10 0	9 2	8 19	5 8 10	8 14	0 1	910 0 0	
15. Ellen Simson....	99	14½	14 0	8 10 0	8 16 0	8 18 0	8 12 0	8 0	8 1	6 13 5	8 12	0 1	891 0 6	
16. ...	65	21½	13 15	13 10 0	13 15 0	13 18 0	13 17 0	13 17	14 1	6 13 5	14 1	0 3	914 17 6	
17. Circassian	60	21¾	13 16	14 14 0	14 2 0	14 15 0	14 12 0	13 12	13 16	6 13 0	14 1	0 6	843 0 6	
18. ... 1299	61	22½	14 16	14 5 0	14 9 0	14 12 0	14 14 0	15 4	14 18	6 14 0	14 1	0 6	927 4 6	
	58	23½	14 18	14 3 0	14 18 0	14 12 0	15 1 0	14 15	14 15	6 14 0	14 18	0 3	874 7 6	16,090 17 6
19. Cuba.—Golconda...	89	13¼	8 0	7 18 0	8 13 0	8 7 0	8 11 0	8 7	8 10	6 13 5	8 1	0 1	769 17 0	
20. ... 263	88	20¾	13 15	12 15 0	12 10 0	9 9 0	13 0 0	13 6	13 12	6 13 5	13 12	6 2	1210 13 0	
21. ...	86	21½	13 14	12 18 0	12 10 0	13 12 0	13 2 0	13 6	13 15	6 13 10	13 15	6 ...	1184 0 0	
22. Gloucester Slag ...	10	5⅝	......	2 0 0	2 10 0	2 16 0	3 0 0	2 0	2 18	0	2 10	0 2 0	30 0 0	3,164 10 0
23. 19	9	27	......	14 10 0	16 10 0	16 12 0	17 5 0	17 5	16 16	0	14 10	0 ...	155 5 0	185 5 0
	1581													19,440 12 6
Each Company's purchase......			147	104	347	......	72½	479½	238	......	193			

The origin of this mode of sale was lately given in the *Morning Herald*, from which we extract the following notice of it :—

"*Origin of the Ticketing for Copper Ores.*—About the year 1700, some merchants at Bristol bought the Cornish ores at prices varying from £2 10*s.* to £4 per ton. About twenty years afterwards, other parties, at the same place, covenanted with some of the principal mines to buy all their copper ores for a term of years at a stated price. About the years 1725–27, great quantities of copper ore were raised from three mines—Huel Fortune (in Ludgvan), Roskear (in Camborne), and Pool Audit (in Illogan)—the produce of which mines were to be sold to the few buyers at their own price. The four copper companies then existing were the Brass Wire Company, the English Copper Company, Wayn and Co., and Chambers and Co., who being united and confederated, had it then, as their successors have at present, their own way. They were interrupted by a gentleman from Wales, who visited the country in order to improve his business. At that time, 1400 tons of copper ore, which had been lying unsold at Roskear and Huel Kitty, were offered to him, for which the associated monopolists would give only £4 5*s.* per ton. So contracted were the principles of the miners in those days, that they obliged the purchaser to deposit a sum of money equivalent to the supposed amount of their ores, before they would consent to weigh them off at the advanced price they agreed to take. 1400 tons of ore were purchased at the advanced price of £6 5*s.* per ton, which was paid for in cash ; the returns on this were over 30 per cent. What must have been the profits of the companies confederated to serve their own interests without limitation or control ? This new comer then purchased 900 tons more at Roskear, at £7 per ton ; and in less than six months before he left Cornwall, he purchased 3000 tons, on which it is supposed he made a profit of 40 per cent. Soon after this the buyers and sellers mutually agreed to ticket for all copper ores which should be ready for sale at stated periods, and the highest bidder or ticket should be the purchaser. On the very outset of this compact, 300 tons of ore, belonging to the same mine, were to be ticketed for in Redruth, when the agent of the mine having absented himself beyond the limited hour of sale, a certain gentleman, of great address, power and fortune, declared himself the purchaser, by private contract, at £8 17*s.* per ton, when one of the ticketers produced his ticket before all the company, whose offer was £9 17*s.*, to the shame and confusion of all the adventurers. To this nefarious system is to be ascribed the present mode of ticketing. The proprietors found themselves in a distressing and ridiculous predicament, possessing a commodity whose value they could not ascertain ; and the

buyers formed themselves into a confederacy, the most pernicious and destructive to the mining interest. The secret transpiring, other companies were formed, and a better price was given for the ore, yet far beneath its just value. At the ticketing day then (as now) a dinner was provided at the expense of the mines, in proportion to the ores they had on sale; and the system appears to have experienced but little modification since it was first introduced."

Formerly another column was in the ticket, marked *standard*, giving the relative value, which indicated the rise or fall of the ore at once. To obtain this, some arbitrary sum was fixed as the smelter's cost for obtaining the copper, and the standard then deduced from the price given; but the constant misapprehension of the standard led to the suspicion, that it was a mere scheme to puzzle and lead the miners astray as to the real value of their ores. The fixing of a standard for general comparison was exceedingly useful for all parties. The true standard cannot be fixed, each smelter having his own standard, and this is always varying, as a rise or fall in coals, wages, &c. will change it; and this every smelter keeps to himself, which is the safeguard of the miner. Notwithstanding, from long experience and a sameness in working, with attention to the market, an average rate may be easily attained for bidding at the ticketings. The following may be given as near to that rate at present, and also the method of procedure for fixing the price to be offered for the ore:—

1. Fix the price which you determine the copper contained in the ore shall yield when delivered into the works; say, for example, you fix upon £65 per ton, and the ore to be bid for has $14\frac{1}{8}$ per cent. Multiply the per-centage by the price, and divide by 100:—

$$65 \times 14\frac{1}{8} = 918 \div 100 = £9 \ 3s. \ 7d. \text{ per ton of ore.}$$

But there are 21 cwts. given to the ton, and other general allowances amounting to 7 per cent. to be added, making—

$$13s. \ 6d. + £9 \ 3s. \ 7d. = £9 \ 17s. \ 1d.$$

2. From the above sum is now to be deducted the returning charges, which are the net cost that practice has shown for smelting such an ore, and this differs with different smelters, but for illustration we keep by the rule. An ore of 9 per cent. produce cost 22s. per ton smelting; 1s. per ton is added for every per cent. above, and 1s. deducted for every per cent. below 9. Thus $14\frac{1}{8}$ per cent. will cost 27s., which deducted from the £9 17s. 1d. leaves £8 10s. 1d. as the sum to be bid for the ore. A little less or more is given according to requirement. Thus the purchaser is guided by the general standard deduced from past sales; and if he, by any improved method or otherwise, smelts at a lower rate, the profit goes into his own pocket.

The ores, when bought, are taken from the place of purchase

to the smelting-house at the cost of the purchaser.   They are all carried to Swansea or the neighbourhood, where almost all the smelting-works are placed.

The ores coming from Ireland, Wales, and abroad are taken to Swansea, and there crushed, sampled, and sold in the same manner as described for Cornwall.   There are now generally two sales each month in Swansea ; and nothing can show better the great increase of imports of copper ore into this country than a reference to the sales at Swansea.   These sales began about the year 1815 ; and according to an interesting table lately published by Mr. Polkinghorne, there was sold in that year £19,203 worth. The average money value of these sales for five years, since 1819 to 1848, stands as under :—

From 1819 to 1824	. .	£33,713
From 1825 to 1830	. .	82,792
From 1831 to 1836	. .	163,785
From 1837 to 1842	. .	628,622
From 1843 to 1848	. .	750,403

The increase of the money value of the ores sold in Cornwall is about one-third in the same period of time :—

> 5 years ending 1824, being £742,508
> 5 years ending 1848, being   999,529

The methods by which the ores are assayed will form the subject of the next communication.

VIII. *Second Report on Observations of the Aurora Borealis, 1850–51, made by the Non-commissioned Officers of the Royal Artillery, at the various Guard-rooms in Canada.   By* Captain LEFROY, *R.A., F.R.S.**

REGISTERS of Aurora have reached me from the following quarters :—

			Latitude.	Long.
			° ′	h m
a Peel's River.........	Oct. 1850 to Apr. 1851 ...	Mr. A. Peers	67 27 N.	8 58 W.
a Youcon	January to May 1851......	Mr. Hardisty.........	66 0	9 48
a Fort Good Hope ...	November 1850	Mr. McBeath	66 16	8 34
a Fort Confidence ...	Oct. 1850 to Apr. 1851 ...	Dr. Rae...............	66 54	7 55
a Fort Simpson	Oct. 1849 to May 1850 ...	Dr. Rae...............	61 51	8 6
a Fort Simpson	September 1850	Mr. Bernard Ross.		
a Pelly and Lewis ...	Dec. 1850 to Apr. 1851	Messrs. R. Campbell and Stewart	61 30	8 40
a Fort Chipewyan ...	Nov. 1850 to Apr. 1851...	Mr. J. Anderson ...	58 43	7 25
b Moose Factory......	June 1850 to Mar. 1851...	Mr. Clouston.........	51 10	5 24?
b Martin's Falls	Sept. 1850 to Mar. 1851 ..	Mr. Wilson	51 52	5 47
b Nipegon	1842 to April 1850.........	Mr. J. Anderson.		
b Matawagomingeu ..	July 1850 to Mar. 1851...	Mr. Colin Campbell.	47 30?	5 28?
b Michipicoton	Nov. 1849 to July 1851...	Mr. Swanston	47 56	5 40

* Communicated by the Author.   For First Report, with Instructions for Observing, see Phil. Mag., June 1850.

To each and all of these gentlemen, as well as to those who may have kept journals which have not yet reached me, I beg to tender my warmest thanks. Nothing can exceed the care and attention displayed by many of the registers, and their interest has fully equalled my expectations. Without meaning to draw invidious comparisons, I cannot deny myself the pleasure of especially naming here those of Mr. Swanston, Mr. Clouston, and Mr. Anderson; the first of these is a model of completeness and conciseness, Mr. Swanston having generally recorded the state of the sky and the weather every hour from dark to 10 P.M., and in terms which are always definite and expressive.

The registers have been continued at the Military Guardrooms of the Royal Artillery in Canada, and at a great number of stations of observation in the United States. I have now in my hands, through the kindness of Professor Henry, Secretary to the Smithsonian Institution, returns from upwards of a hundred observers, for 1849, 1850, and part of 1851, at stations scattered through all the States, from the Atlantic to the Mississippi. Not having received observations from any of the stations on the Saskatchawan or Lake Winnipeg, there is a pretty wide blank, extending from Lake Athabasca to Lake Superior, in the chain by which it was hoped to trace and identify displays from the polar circle downward to Canada, but I trust in future years some at least of the intermediate posts will oblige me with a journal; and if each observer will bear in mind that others, hundreds, and some of them thousands of miles off, are noting down the features of the very displays he may be looking at, as it appears to them, and that from a comparison of all these accounts it is hoped to arrive at definite views concerning this most singular phænomenon, he cannot fail to see the value which every clear, distinct, and definite record of facts and particulars will possess, and to acquire a greater interest in the subject than the constant repetition of familiar descriptions might otherwise afford.

It has been often stated vaguely that aurora appears every clear night. This is certainly not true of any one station, as far as the earlier hours are concerned; we are still short of proof that it is true in the widest meaning; indeed, the statement, if true, would carry little weight with it without the addition of dates, facts and particulars. These, however, our registers promise, for the first time, to supply. Observations begin to be general in October 1850. In that month we have evidence of it every night except five, 20, 21, 22, 23, 26, one of them clouded everywhere, one of them full moon, the rest partially clouded. In November 1850, every night but two, 22 and 23; the former, however, of these was generally clear and no moon.

In December 1850, every night but five, 5, 10, 18, 19, 20, but all the displays of a feeble character.   In January 1851, every night but two, 5, 12; many of the displays very feeble, several of them seen only by Mr. Anderson at Athabasca, and on the whole a much smaller proportion than usual, extending to low latitudes.   In February we have it every night*, some of the displays of great beauty, although I imagine they will have been far exceeded by those of February 1852.   The display of February 18th, 1851, was one of those remarkable instances of the simultaneous *absence of cloud,* and intense development of aurora over a very large part of the northern hemisphere, which, from their frequent occurrence, appear to have more than an accidental connexion.   It was seen at every station, with exception only of the Pelly Banks, from the polar circle to the United States, where no less than thirty-eight stations have forwarded accounts of it to the Smithsonian Institution; it extended also to Europe, having been recorded at Sandwick Manse, Orkney. The display of February 28th was almost as universal.   It is remarkable that in both cases the phænomenon was first seen, in absolute time, at the most eastern stations, notwithstanding the earlier commencement of darkness at the extreme north, where the difference of latitude in some cases more than compensates the difference of longitude; it would appear from this that the aurora does not commonly appear at a station upon any meridian until that meridian generally is in darkness; a result, which, if established by the whole body of evidence, will be both new and interesting.   For example, in the following list I have entered the hour of sunset in mean time of Göttingen at each station, and the hour at which the aurora is first recorded in the same; it is not to be supposed that each observer seized the exact time of first visibility, but in two of the examples at least the general result is sufficiently clear, namely, that it was seen at the lower and eastern stations sooner than at the northern but more westerly stations, although there is no reason to be given why it should not have appeared at the latter as soon as at the former, daylight having ended there.

* Every night but two, February 2 and 16.

Table I.

	N. lat.	W. long. from Greenwich.	January 27, 1851. ⊙ Dec.−18½°. Eq.−13 m.		Feb. 18, 1851. ⊙ Dec.−11½. Eq.−14 m.		Feb. 28, 1851. ⊙ Dec.−8. Eq.−13 m.	
			Sunset. Göttingen.	Aurora first seen.	Sunset. Göttingen.	Aurora first seen.	Sunset. Göttingen.	Aurora first seen.
	° ′	h m	h m	h m	h m	h m	h m	h m
Toronto	43 39	5 17	10 56	19 0	11 26	14 0	11 39	16 0
Halifax	44 39	4 14	10 1	......	10 18	11 49	10 15	
Quebec	46 49	4 45	10 14	14 25	10 35	12 24	11 4	15 35
Newfoundland ...	47 33	3 31	8 58	......	9 32	20 11	9 48	12 40
Michipicoton	47 56	5 40	11 6	......	11 50	16 40	11 57	13 55
Moose Factory ...	51 10	5 24	10 39	14 54	11 17	12 44	11 37	12 49
Martin's Falls......	51 52	5 47	10 58	14 30	11 36	13 1	12 0	13 1
Athabasca	58 43	7 25	12 3	14 30	13 16	15 55	13 24	15 25
Lewis and Pelly...	61 30	8 40	13 8	21 30	14 16	......	13 41	15 58
Fort Simpson	61 51	8 6	12 21	......	13 15			
Youcon	66 0	9 48	13 21	20 0	14 56	18 58	15 27	18 3
Fort Confidence...	66 54	7 55	11 21	17 0	12 55	16 25		
Peel's River	67 27	8 58	12 19	16 38	13 50	17 38	14 31	at dark.

I do not offer these instances as conclusive, but they are somewhat remarkable; and I may state, that, having marked the Göttingen hour of the first appearance against every observation, the great majority give direct support to the inference I have drawn, and there are few or no instances contradicting it. The question will soon be decided if the time of commencement of each display is recorded, and a note also made of the latest hour at which it may have been noticed that there was no aurora. For example, the observer takes a look out at 7 P.M., no aurora; again at 8, aurora, which is duly entered. In this connexion, the fact that there was no aurora at 7 is almost as important as the fact that the phænomenon was visible at 8, and should be duly entered. On the 29th of September, 1851, at 6ʰ 30ᵐ P.M., there was no trace of aurora at Toronto; at 6ʰ 36ᵐ, a brilliant, heavy serpentine band occupied the northern sky. In this instance, and in various others, the time of appearance is fixed to five or six minutes, and if at any northern station it happens to have been fixed with anything like the same exactness, the question will be answered.

In March 1851, we have evidence of aurora every night save three, 13, 17 (full moon), 19; these, however, were pretty generally clear nights. Registers for April 1851, have reached me from a few stations only, but as far as they go give evidence of aurora every night save four, 4, 14, 15 (full moon but clouded everywhere), and 21. The 16th of December is the only instance

in the winter of aurora seen in Canada which escaped notice at every northern station; the number seen at northern stations which do not descend to Canada is of course considerable, as will appear from Table III.

Table II.—Showing the number of nights the aurora is recorded at each station in 1850 and 1851, and the total number of nights in each month in which there is evidence at present to show that the phænomenon was developed somewhere or other on the American continent. The returns will, no doubt, be extended, and some observations at present omitted as doubtful be confirmed, and included in the totals at certain stations.

1850.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
a Peel's River	...	...	...	...	...	...	...	...	...	6	11	9	
a Fort Good Hope	...	...	...	...	...	...	...	...	...	...	20		
a Fort Confidence	...	...	...	...	...	...	...	...	...	12	9	15	
a Fort Simpson	3	5	5	0	*	*	*	*	8e	...	...	...	
a Lewis and Pelly	...	...	...	...	...	...	...	...	...	...	4		
a Lake Athabasca	...	...	...	...	...	...	...	...	...	...	12	19	
b Martin's Falls	...	...	...	...	...	...	...	2f	10	10	10	11	
b Moose Factory	...	...	...	...	...	7a	18	19	12	15	11	13	
b Matawagomingen	...	...	...	...	...	...	3	7	4	4	2	0	
b Lake Nipegon	...	5	6	7†	...	...	...	...	...	...	...	...	
b Michipicoton	3	4	3	7	1	6	5	2	1g	3	2	2	39
c Newfoundland	1	6	6	1	3	3	8	6	6	9	0	3	52
c Quebec	0	2	3	3	1	3b	6c	6c	7c	5c	5	3	44
c Montreal	1	3	5	5	3	...	...	...	...	...	...	...	
c Halifax	1	4	6	4	3	6	13	10	9	11	8	4	79
c Fredericton	...	...	...	...	...	...	4	4	5	5	9		
c Kingston	0	4	1	3	0	0	6	3	5	5	1	0	28
c Toronto	3	3	3	6	5	3	5.	4	5	9	3	1	50
c London, C. W.	1	3	3	5	3	0	3	2	5	5	0	0	30
c Somerville, N. Y.	0	7	9	8	6	8	9	14	12	7	3	4	87
Total	11	23	23	19	13	21	27	25	21	25	27	26	261

(*a*) From 16th to 30th June. (*b*) Observed by Sergeant Maiden at Grose Isle, near Quebec—none observed at Quebec. (*c*) Including observations at Grose Isle. (*d*) Begins on the 30th. (*e*) From 1st to 18th. (*f*) Begins on the 30th. (*g*) Begins on the 21th.

* Twilight too strong.   † Register ends.

Table II. (*continued*).

1851.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
Peel's River	10	16	12	1	*								
Fort Confidence...	11	15	12	4b									
Youcon	7a	6	6	5	*								
Lewis and Pelly...	8	12	10	2c									
Athabasca	20	19	20	19									
Martin's Falls.....	10	12	14										
Moose Factory ...	14	15	17										
Matawagomingen	0	3	3										
Michipicoton......	2	6	7	9	5	2	5						
Newfoundland ...	4	6	6	5	7	1	1	3	5	6	2	5	51
Quebec	3	3	3	3	1	1	3	0	5	5	0	1	28
Montreal													
Halifax	4	5	8	9									
Fredericton													
Kingston	0	1	5	0	1	0	2	2	6	2			
Toronto	2	3	7	2	8	4	5	7	9	7	5	9	63
London, C. W. ...	1	2	3	0	0	2	1	2	4	1	0	2	18
Somerville, N. Y.	0	3	5	6	6	2	6	3	9	7	1	5	53
Total.........	26	26	28	26	25	9	15	11	19	13	6	13	207

(*a*) Commencing on the 19th. (*b*) Down on the 11th. (*c*) Down to the 10th.

In these enumerations, doubtful entries are not included unless supported by an observation elsewhere in the same region. I have added the number of observations made by a most indefatigable observer (Dr. Franklin B. Hough) at Somerville, near Ogdensburg, on the St. Lawrence, both as properly belonging to the Canadian chain of stations, and to show that even in low latitudes a single observer, by great attention, may make a surprising advance on the number of instances of aurora, which attract the attention of those who are less zealous or less favourably situated. The stations may be arranged in three groups; the first comprising all those marked (*a*), which are from 500 to 1000 geographical miles distant from the magnetic pole; the second, those marked (*b*), which are from 1200 to 1500 miles distant; and the third, those marked (*c*), including the great majority of stations in the United States, which are from 1600 to 2000 miles distant, from the same point. Lake Athabasca, contrary perhaps to first impressions, is the nearest permanent station to this assumed centre of influence. Fort Confidence, which is not a permanent station, is of course nearer; but Fort Simpson and the other posts on McKenzie's River, notwithstanding their northerly position, are somewhat more distant.

Table III.—Observations arranged according to position of Station with reference to the Magnetic Pole.   The figures under the heading Proportion shows the per-centage of observation of Aurora, to nights on which observation appears to have been possible, as regards the state of the sky.

Date.	(a). In first circle, from 500 to 1000 miles distant.						(b). In second circle, from 1200 to 1500 miles distant.					
	No. of stations.	No. of observations of aurora.	Doubtful appearance.	Totally clouded nights.	Observation probably possible.	Proportion per cent.	No. of stations.	No. of observations of aurora.	Doubtful appearance.	Totally clouded nights.	Observation probably possible.	Proportion per cent.
1850.												
January	1	3	...	12	16	16	1	2	1	19	11	17
February	1	6	...	10	12	33	2	5	...	20	3	62
March	1	5	...	10	16	24	2	7	...	15	8	47
April	1	0	...	13	17	...	2	10	...	14	6	62
May	1	0	...	2	17a	...	1	1	1	14	16	...
June............	1	6	1	4	3	...	2b	10	3	13	7	59
July	...	...	...	...	...	...	3	19c	1	9	3	94
August.........	...	...	...	...	...	...	3	22d	1	7	3	92
September ...	1	8f	...	9	...	100	3	17d	1	7	6	74
October	2	16	...	11	4	79	4	19	...	6	5	79
November ...	4	25	...	3	3	93	4	16	1	9	5	79
December ...	4	25	...	3	3	88	4	16	1	10	5	76
1851.												
January	5	28	...	2	1	96	4	15	...	11	5	79
February	5	25	...	1	2	93	4	17	...	9	2	89
March	5	25	...	1	4	83	4	21	...	4	6	78
April	4e	23	...	2	5	82	1	9	...	16	5	64
May	...	...	...	...	...	...	1	5	...	13	13	28
June............	...	...	...	...	...	...	1	2	...	10	18	10
July	...	...	...	...	...	...	1	5	...	13	13	28

(*a*) Down to the 19th May. There is no night, properly speaking, at Fort Simpson in May; that is to say, twilight lasts from sunset to sunrise. (*b*) Register at Moose Factory begins on the 17th of June, (*c*) including Mr. Anderson's observations *en route*. (*d*) No observations made at Michipicoton from the 21st of August to the 20th of September, but probably no conspicuous aurora occurred. (*e*) Four stations up to the 10th, from 14th to the end, only two. (*f*) Register from the 2nd to the 18th of September.

In the third circle (*c*) or stations from 1600 to 2000 miles from the magnetic pole.

Table III. (*continued*).

	1848.			1849.			1850.			1851.			Average in four years.	Relative number.
	N. C. Officers, Royal Artillery.	Added by other observers.	Total aurora.	N. C. Officers, Royal Artillery.	Added by other observers.	Total aurora.	N. C. Officers, Royal Artillery.	Added by other observers.	Total aurora.	N. C. Officers, Royal Artillery.	Added by other observers.	Total aurora.		
January ...	8	3	11	8	...	8	5	4	9	8	3	11	9·7	70
February ..	13	1	14	17	...	17	13	6	20	1	6	13	16	127
March ...	13	1	14	11	5	16	12	10	22	15	5	20	18	159
April	17	...	17	20	2	22	12	5	17	13	3	16	18	185
May	11	...	11	10	7	17	10	3	13	9	5	14	13·7	161
June	6	...	6	9	3	12	11	6	18	5	3	8	11	141
July	9	1	10	12	6	18	17	4	21	7	6	13	15·5	191
August ...	6	3	9	8	6	14	15	4	18	8	3	11	13·2	143
September	7	...	7	12	7	19	16	3	19	12	6	18	15·7	148
October ...	14	1	15	12	2	14	17	1	18	12	1	13	15.	124
November	10	...	10	12	2	14	15	3	18	6	...	6	12	89
December	12	1	53	6	4	10	10	2	12	12	1	13	12	85
	126	11	...	137	44	...	154	51	...	114	42			

The figures in the last column are found by dividing the average of auroras in each month, by the number of hours *less one*, from sunset to sunrise at Toronto (taken as a middle latitude), on the 15th of that month; they show in a striking manner the diminished frequency of the phænomenon about the winter solstice, and its great development at the vernal equinox. The returns for the last three months of 1851 are not all collected.

The stations of observation in the first and second groups are not yet numerous enough to decide the question, whether the aurora ever appears in the exterior when it is absent in the interior circles; but in forming an opinion on the number which extend from the interior to the exterior of them, we must not forget, that, notwithstanding the large number of observers, both regular and occasional, in the third group, and their wide distribution, a considerable proportion of the entries in Table III. rest at present upon an observation at only one station*; and *unless particulars are given,* which is unfortunately not always the case, may be reasonably regarded as doubtful. Where particulars are given, there can be of course no doubt.

The observations made under the direction of the Smithsonian Institution begin to be general in March 1849; and the stations are so numerous, that we ought perhaps to consider observations to have been possible every night. Table III. has been made as complete as possible, by including some observations kindly

* Of the total number of 261 observations in 1850, 54 are at one station only; of the total number of 207 in 1851, 71 are at present at one station only—the majority of these in the third group. The proportion to which any doubt can attach is not large.

communicated by Mr. E. C. Herrick, together with any that were found in the Regent's reports for 1848–49, of which particulars were given, or which occurred at more than one station on the same evening. Also observations by Mr. Dougald Stewart at Ristigouche, L. C. The few observations at sea at present collected, for most of which we are indebted to Captain Oliver Eldridge, have not yet been included.

It results from the comparison of the six winter months, October to March inclusive, 1850–51, that aurora was seen before midnight within the first circle on 88·5 per cent. of practicable nights, in the second circle on 80 per cent., and in the third on only 48·5 per cent., indicating a rapid falling off of the causes producing it at distances exceeding 1600 miles from the magnetic pole.

It is scarcely necessary to say, that these simple numerical comparisons are but the first fruits of the observations; such as they are, however, they suggest to the mind a spectacle, which, if true in nature, must be of wonderful magnificence. The polar light kindling on each meridian as that of day declines, sometimes with the splendour of prismatic colouring over half a hemisphere; sometimes contracting its circles and paling its fires, for a period of days or weeks; and sometimes spreading downwards over the globe with an intensity of which our highest conceptions are probably most inadequate, since, if the region of the display is as elevated as is usually supposed, about a third of its light must be absorbed by the atmosphere. To pursue the subject into all its details would lead me much beyond the limits of such a communication as this; but I am truly anxious to convince any gentleman who may have doubts on the subject, that to keep, in ever so plain a way, a journal of such appearances as may occur at his station, will be a most acceptable contribution to an inquiry which will owe much of its interest and value to the scale on which it is pursued; and especially to induce those to whom I have not the advantage of being personally known, and those resident at the remaining posts in the northern, middle, and extreme western regions, to swell the list.

With respect to the influence of these displays upon the movements of the magnetical elements registered by photography at Toronto, I may say that I find the symbols which represent, in the abstract, 'total absence of disturbance,' 'moderate disturbance,' 'considerable disturbance,' and so on, against almost every variety of observation, and am not yet prepared to give any settled opinion on the subject.

I cannot close this letter without referring to the great value of such observations as the following by Mr. Hardisty; which, probably, but for this attempt to follow up the phænomenon to its fountain-head, would never have been added to the very few

and much-disputed observations of the same nature which are on record. That gentleman writes,—"During a voyage in the beginning of the past winter, I saw the most beautiful display of aurora borealis that I believe I ever witnessed. On the 2nd of December 1850, I encamped on the banks of a moderate-sized river near the chain of the Rocky Mountains running westward of Peel's River, the opposite banks rising precipitously several hundred feet until they joined the mountains beyond. Having no time-piece with me, I cannot speak positively as to time; but it would have been probably between 10 and 11 P.M., with a fresh breeze blowing from N.E. and very cold. The phæno-menon *was evidently very near the earth, for it appeared between me and the trees on the opposite side of the river, which could not have been* 40 *feet above the level of the stream; the trees toward the top of the hill being high above it.* Large compact masses were moving from E. to W., and bright streamers passing in the same direction in quick and vivid flashes; then returning to the zenith, would from thence spread out to the N. and S. in beau-tiful waves or clouds, and sheets of light of the most beautiful colours, until they disappeared altogether and left the sky en-tirely clear. Every time the streamers passed over me *from* E. to W. they were accompanied by a rustling noise, such as would proceed from the gentle waving of a *silk* flag; but in returning from W. to E. I am not conscious of having heard any sound proceed from them." [The Italics are the writer's.] It is a con-firmation of the very remarkable proximity of this display to the observer, that the following are the only other observations on the same evening, although it was clear at four or five stations :—

" *Fort Confidence.*—A very faint band of aurora near horizon in the N. at 5 P.M.; at $7^h 30^m$ it formed a pale arch across the zenith from N.N.W. to S.S.E.; at $8^h 50^m$ it exhibited a broad arch from N. by W. to S.S.E., altitude towards S.W. about 9° at vertex (true bearings). *Moose Factory*, at $8^h 40^m$, a faint auroral light in N.; $9^h 20^m$ brighter, but partly obscured by clouds; 10^h still visible, never higher than 30°."

Mr. Bernard Ross remarks of a splendid display on the 1st of February 1850, that "although *very* near the earth, no sound was audible," but does not mention on what grounds he supposed it to be so near.

I shall look with much interest for the observations made in the past winter, which in Canada has been remarkable for the number of splendid displays of aurora, and the repeated occur-rence of some of the rarest phænomena connected with it, such as the formation of arches of *dark* vapour, of which Mr. C. Campbell has given one instance.

Magnetical Observatory,
Toronto, April 13, 1852.

IX. Notices respecting New Books.

Ædes Hartwellianæ, or Notices of the Manor and Mansion of Hartwell. By Captain W. H. Smyth, *R.N.*

THE greater part of this volume, the original object of which appears to have been to give an account of the Hartwell Observatory, is occupied with topographical and statistical details respecting the parish and manor of Hartwell, historical notices of the successive Lords of the Manor, and particulars relating to Hartwell House, its apartments, paintings, library, museum, numismata, and Egyptian antiquities. It would be out of the province of a scientific journal to make observations on the subjects of this " Miscellany ;" but we may be allowed to say that the work abounds in interesting matter, treated with singular liveliness of style and great variety of erudition, and that it is beautifully illustrated by plates engraved from drawings expressly made by the talented members of the author's family. Our remarks must be restricted to the contents of Chapter IV., which is exclusively scientific, embracing the following astronomical subjects :—origin and description of the Hartwell Observatory ; meridional observations; measures of double stars; colours of the same ; the story of γ Virginis ; Encke's comet.

The *Hartwell Observatory*, which is the property of Dr. Lee, the present Lord of the Manor of Hartwell, whose love and patronage of astronomical science are well known, originated as follows :— " In December 1828," says Captain Smyth, " soon after I had completed my observatory at Bedford, and mounted the instruments lent by the Astronomical Society for that purpose, it was communicated to me that the telescopes, clock, transit-circle, portable transit, and numerous other articles, which had belonged to the late Rev. Lewis Evans, were to be disposed of by private sale. On viewing them I was rather chagrined at the circumstance not having occurred before my arrangements were carried into effect ; especially as the circle seemed to me greatly superior in simplicity and efficiency to Colonel Beaufoy's, with which I had just commenced operations. On mentioning this to Dr. Lee in the evening, he resolved to make the purchase, and to present the circle to the Astronomical Society, with the understanding that it was to change places with the one at Bedford; a transaction which accordingly took effect." The transit-circle being thus disposed of, the small transit-instrument and a reflecting telescope were mounted on pedestals at the south portico of Hartwell House, and the clock with the rest of the instruments were located in an adjoining strong-room. The principal results of this incipient observatory were to create a desire in Mr. Thomas Dell of Walton, near Aylesbury, to possess also a clock and transit-room (subsequently erected under Captain Smyth's superintendence), and to inspire Dr. Lee himself with the wish to procure more powerful astronomical apparatus. Accordingly in 1831 a transit-room was built at the south-east angle of the mansion, for the reception of a five-foot transit telescope, to be employed especially in observations of the moon and moon-culminating stars for the determination of

terrestrial longitudes. The room is eighteen feet by twelve, sixteen feet in height outside, and ten feet five inches inside, and has a flat and well-leaded roof. The stone piers, six feet high, and cut from a single block of Portland stone, are erected on a brick foundation resting on the "live" rock, and the flooring of the room is carried so as not to touch them. The transit-clock, by Vulliamy, has two peculiarities suggested by Captain Smyth: the steel rod of the pendulum is immersed six inches in the mercury, that both may be simultaneously affected by changes of temperature; and the clock-weight consists of separate cylindrical pieces, by which the moving force may be adjusted so as to produce any required arc of vibration. Two meridian marks (mounted, characteristically of our author's antiquarian predilections, one on a representation of the temple of Janus, the other on a miniature of the façade of the Temple of Concord at Girgenti) are placed respectively at the distance of one hundred feet north and south of the observatory slit, and by the intervention of two lenses of one hundred feet focal length fixed in the window-sills, are viewed by parallel rays entering the transit telescope. This meridian appliance, the theory of which (as we gather from the statement at the top of page 236) was suggested to the author by Baron de Zach, has the great advantage of enabling the observer to ascertain at all times the error of collimation of his telescope, without waiting, as in the use of a distant meridian mark, for a favourable state of the atmosphere. The method of two collimators looking into each other, which is that now employed at Greenwich, involves the same principle, and has the further advantage of not even requiring a reversion of the transit.

Three years after building the transit-room, Dr. Lee determined upon enlarging his astronomical means by the addition of an equatorial. Under Mr. May's able engineering, a tower, solidly built, and of fifteen feet interior diameter, was surmounted by a hemispherical dome, covered with copper sheathing, moveable on three cannon-balls, and opening by a single shutter extending from the zenith to the wall-plate. After some delay, occasioned by the object-glass purchased for the equatorial being pronounced by Mr. Dollond to be unworthy of a costly mounting, it was arranged that the telescope employed by Captain Smyth in making the observations recorded in his "Cycle of Celestial Objects," being no longer in use, should be transferred from Bedford to the Hartwell Observatory. The object-glass, one of Tulley's best, is 5·9 inches in diameter, and of 8 feet $8\frac{1}{2}$ inches focal length. The equatorial is mounted in a very simple manner, has hour and declination circles each 3 feet in diameter, and is moved by clock-work.

The *meridional observations* taken by Mr. Epps, late Assistant-secretary of the Royal Astronomical Society, were begun in the early part of 1838 and continued to August 1839, when they were interrupted by the death of the observer. The observations of 315 of the stars, many of them taken with the moon, are discussed by Captain Smyth, and absolute right ascensions deduced from them are compared (pp. 256–283) with the Astronomical Society's Cata-

logue, for the sake of testing their trustworthiness in the determination of longitudes, the object to which the observations were primarily intended to be applied. In fact, while absolute determinations of celestial positions can only be effectively made in large public astronomical establishments, the means of private observatories may be most usefully employed in *differential* observations, in which class moon-culminations, limited to the application just mentioned, are to be included. In 1842 the observatory was trigonometrically connected by Captain Smyth, and his son Henry Augustus, of the Royal Artillery, with Aylesbury church-spire, and by inference from the great Trigonometrical Survey, its longitude was found to be $3^m 22^s\cdot 63$ west of Greenwich, and its latitude $51° 48' 14''\cdot 6$ north. There appear to be ample means of verifying by independent astronomical observations the assumed position of Aylesbury Church, as no fewer than three observatories furnished with transit-instruments, in addition to that of Hartwell, exist in the immediate neighbourhood; Mr. Dell's, already mentioned, one erected by the Rev. J. B. Reade at Stone, and another by the Rev. C. Lowndes at the Hartwell Rectory.

The *measures of double stars* (pp. 287–290) were taken with the equatorial of the Hartwell Observatory, the instrument being obligingly kept in readiness by Dr. Lee for the immediate and particular service of its former possessor. They are re-examinations of objects enrolled in the Celestial Cycle, and being made by the same observer and the same instrumental means, are strictly comparable with the measures recorded in that work.

Captain Smyth has paid particular attention to *the colours of double stars*. In the work before us we are presented with a comparison, probably the first of the kind, of two independent series of observations of this class. It appears that the Bedford Catalogue, in which such colours are assigned to all the objects as struck the observer at the time of observation, reached the hands of Signor Benedict Sestini of Rome, after he had been engaged on a very extensive series of observations of the colours of stars in general, and led him to form a table of comparisons of his own estimates with those of Captain Smyth on double-stars. The conclusions he had already arrived at, which for their interest deserve to be mentioned here, were, that " of 2540 stars (those of Baily's Catalogue observed at Rome) the yellow stars are about half the total number, and equally distributed ; the white stars are one-fifth in scattered portions ; and the orange rather more than one-fifth. The red and the blue are rare from the pole to 30° of north declination ; the blue then become numerous $(=\frac{1}{7})$ to the equator, especially from Æ. 18^h to 20^h; and the red abound from 0° to 30° south declination, and Æ. 16^h to 20^h." But it would seem, when the result of the above-mentioned chromatic comparison is taken into account, that such conclusions require for their establishment the collective observations of different observers in different circumstances. Pages 293–298 contain a table of the colours assigned to the components of 109 of the brighter double-stars by Smyth in the years 1831–43, by Ses-

tini in 1844–46, and again by Smyth in 1849–51. The two lists of English observations agree well enough with each other, but differ in a remarkable degree, and in a large number of instances, from that of the observations made in Italy. Some of the discrepancies are adverted to by Signor Sestini in these terms :—" Now, beginning with the comparison of γ Andromedæ, we have Smyth emerald-green, and Sestini white ; but Herschel and Struve at another date call it azure. Moreover, observing it again after a lapse of two years, and four years after Smyth, I find it no longer white, but a strong blue! " " Now see B(95) Herculis : according to Smyth, one is greenish and the other red ; but we think them both a golden-yellow ; A Ophiuchi, by Smyth, one ruddy and the other pale yellow ; but we take them to be both orange. The contrary occurs in ι Bootis, the components of which by Smyth are both pale yellow ; but we deem one to be orange and the other azure."

What can be the cause of such differences ? " The disagreements between the tints of stars as given by Sir William Herschel and myself," says Captain Smyth, " are partly accounted for by his peculiarity of vision and the tone of metal in his reflectors. But I am at a loss why refractors should differ so widely as here shown ; and therefore hope the subject will be more closely pursued than it has hitherto been." The explanation proposed by Signor Sestini, viz. that the colours of the stars vary in consequence of variations of their velocity, is not admissible. Neither are we prepared to take the view which Captain Smyth appears to advocate, viz. that very minute differences in the velocity of transmission of rays of different colour, cause variations of the colours of stars. Certainly on this hypothesis, if a new star suddenly appeared in the sky, its existence would be announced at successive epochs by the different rays of its spectrum, and its colour would not be permanent till the rays had all reached our position in space. Changes of colour in the reverse order would occur on the extinction of a star. On the same hypothesis, variations of colour would accompany variations of brightness. But such variation of colour has not hitherto been detected in stars that notably vary in brightness. It seems probable that the discrepancies in the estimates of the colours of stars are due to various sources of error in judging of tints, which after all form but a small portion of their total light ; such, for instance, as the general state of the atmosphere at the time of observation ; the effect of altitude above the horizon ; the effect on the eye of the observer of the artificial light used for the purposes of observing. Possibly, also, the achromatism of the object-glass, which, being adapted to the solar spectrum, may not be suitable to the spectrum of the star, ought to be taken into account ; as well as the necessity of a nice adjustment of the eye-piece for eliciting the proper colour of each star. " Chromatic personal equation," that is, the faculty in a greater or less degree of appreciating differences of colour, must be a fruitful source of discrepancy. Many valuable hints are given by Captain Smyth (pp. 306–310) towards obviating some of these sources of error, and towards fixing upon a standard scale of

reference in the chromatic observation of stars. Considering, however, the many difficulties that beset this inquiry, it is impossible not to feel the force of Sir John Herschel's assertion, that " nothing short of a separate and independent estimation of the total amount of the red, the yellow, and the blue rays in the spectrum of each star would suffice for the resolution of the problem of astrometry in the strictness of its numerical acceptation ; and this the actual state of optical science leaves us destitute of the means even of attempting with the slightest prospect of success." (P. 301.) Perhaps an approximation by instrumental means to the spectra of the brighter stars ought not to be despaired of. An instance is adduced (p. 299), in which Sir David Brewster accounts for the orange colour of the double star ζ Herculis by an analysis of its light.

The "*Story of γ Virginis*" is one of great interest, this being perhaps the most remarkable instance in which the components of a binary star have been shown, by the combination of theoretical calculation with observation, to be acted upon by their mutual attractions. Herschel, Encke, Mädler, Smyth, Henderson, Hind and Adams, are all astronomical names that have been enlisted in the theoretical investigation of the orbit of γ Virginis. But no astronomer has so diligently observed this object as Capt. Smyth. His observations extend over the twenty years commencing with 1831. In the month of January 1836 he pronounced it to be *round,* and in April and May of the same year saw it *elongated.* Sir John Herschel, in a letter from the Cape of Good Hope under the date of Feb. 27, 1836, says, " γ Virginis, at this time, is to all appearance a single star." The observations that have been employed by the theoretical calculators, reach as far back as 1718. In that year Pound assigned the relative position of the two stars by allineation with a known star seen with the eye directed to the sky, while the other eye was looking through the telescope. In the years 1719 and 1722 Bradley made like observations. This mode of observing, as Sir John Herschel has shown, requires a correction for a kind of optical equation between the judgements of the two eyes. Other observations were made by Mayer, in 1756 ; Herschel I., in 1781 and 1803 ; Herschel II. and South, in 1822 ; Struve and South, in 1825 ; Herschel II. and Struve, in 1828 and 1829 ; Herschel II., in 1830 ; and Dawes, in 1830 and 1831, which brings us to the date of Capt. Smyth's observations. Subsequent to these there are observations of Dawes, Lord Wrottesley, Mr. J. Fletcher of Cockermouth, and Mr. J. F. Miller of Whitehaven.

Sir John Herschel attacked the theoretical problem in an admirable and well-known communication to the Royal Astronomical Society, inserted in vol. v. of their Memoirs. He uses measures of distance, on account of their uncertainty, only for the determination of the major axis, making the values of all the other elements depend on measures of angular position. The method is in other respects essentially graphical, " the aid of the eye and the hand being brought in to guide the judgement in a case where judgement only, and not calculation, can be of any avail." The first essay gave

a periodic time of 513 years. It is, however, to be remarked that after the date (1832) of that communication, the stars went through a critical part of their relative orbit, and subsequent observations were more suited to an exact determination of the periodic time. Sir John Herschel afterwards stated the period to be short of 150 years. Mädler found 145 years, Henderson, 143. Finally, in the volume of the Cape observations, Sir John Herschel entered upon a re-investigation of the orbit, and concludes the research with the following summary :—" Comparing the orbits which seem entitled to most reliance, it appears certain that the eccentricity lies between 0·855 and 0·880, the inclination between 23° and 27°, the perihelion epoch between 1836·20, and 1836·45, and the period between 140 and 190 years." It may here be remarked that the apparent eclipse of one star by the other which was observed in 1831, was not owing to the passing of the plane of the orbit through the position of the spectator, for all the calculations concur in giving a small inclination of that plane to the surface of the celestial vault; but to an actual approach of one star to the other, for the calculations as uniformly assign a large eccentricity to the relative orbit. Such an approach must have enormously changed the thermotic relations of the two bodies to each other.

It will be an appropriate conclusion to this account to put in juxta-position Sir John Herschel's last elements, the elements obtained by Mr. Hind exclusively from Capt. Smyth's observations, and those of Mr. Adams, which take for basis Sir John Herschel's orbit, and are formed on the principle of distribution of errors by the method of least squares.

ELEMENTS OF γ VIRGINIS.

	Herschel.	Hind.	Adams.
Perihelion passage	1836·39	1836·40	1836·34
Ascending node...........	28° 42′	20° 34′	34° 45′
Position at Perihelion	322° 12′	323° 50′	
Inclination to plane of projection	30° 39′	27° 23′	25° 27′
Distance of Perihelion from node	290° 30′	300° 13′	284° 53′
Excentricity	0·8860	0·8804	0·87964
Period of revolution	years. 183·14	years. 171·54	years. 174·137

The astronomical portion of the work concludes with a dissertation on comets, accompanied by a representation of *Encke's comet*, as it was seen by Professor C. Piazzi Smyth with the Hartwell Telescope, at its reappearance on the 22nd of September 1848. This comet, like Biela's and others, seems to be entirely gaseous, and of such tenuity of substance that the smallest stars are visible through it without sensible diminution of their brightness.

X. *Intelligence and Miscellaneous Articles.*

ON THE COMPOSITION OF HUMAN FAT. BY DR. HEINTZ.

THE fatty acids procured in the form of a soft mass by the decomposition of soap prepared with human fat, were pressed as much as possible, and the residue dissolved in a third part of its weight of boiling alcohol; the mass procured by exposing this to as low a temperature as possible was again pressed, and this process repeated until no trace of oleic acid was to be found in the remaining solid acids. This mixture of solid acids was analysed by repeated precipitation with acetate of lead, and four different acids procured from it.

The first of these acids, which is most readily precipitated in combination with oxide of lead, exists only in very small quantity; from about $2\frac{1}{2}$ lbs. of human fat only about $0 \cdot 2$ grm. were procured in an apparently pure state. It crystallized from the alcoholic solution in small, transparent laminæ of a pearly lustre; on fusion it solidified into peculiar scale-like crystals. Its melting-point is at 156°, and was not raised by repeated crystallization from alcohol. Its analysis led to the formula $C^{36} H^{36} O^4$.

	Found.		Calculated.
Carbon........	75·84	C^{36}........	76·06
Hydrogen	12·70	H^{36}	12·68
Oxygen	11·46	O^4........	11·26
	100·00		100·00

Heintz considers it probable that this acid is identical with the *stearophanic acid* discovered by Dr. Francis[*] in the berries of *Cocculus indicus.*

The second acid, which, next to that above mentioned, is most readily precipitated by oxide of lead, is called *anthropic acid* by Dr. Heintz. From $2\frac{1}{2}$ lbs. of human fat only about 1 grm. of this acid was procured. When pure it crystallizes from the alcoholic solution in beautiful broad laminæ of a pearly lustre, melts at 133°, and solidifies on cooling into beautiful shining laminar crystals. The alcoholic solutions of its alkaline salts solidify on cooling into an opaline jelly; earthy and metallic salts produce insoluble precipitates in these solutions. Dr. Heintz considers the composition of this acid as not yet placed beyond doubt; his analyses gave for the free acid the formula $C^{34} H^{32} O^4$; for the silver salt, $AgO, C^{34}H^{31}O^3$; for the baryta salt dried at 212°, $BaO, C^{34} H^{31} O^3 + HO$.

Free acid.		Silver salt.			Baryta salt.	
Found.	Calc.	Found.	Calc.		Found.	Calc.
C^{34} 75·99	76·12	C^{34} 53·87	54·41	C^{34}	59·23	59·24
H^{32} 12·40	11·94	H^{31} 8·47	8·27	H^{32}	9·35	9·29
O^4 11·61	11·94	O^4 8·78	8·54	O^4	9·03	9·29
100·00	100·00	Ag 28·88	28·78	BaO	22·39	22·18
		100·00	100·00		100·00	100·00

Dr. Heintz considers it possible that this acid may prove identical with the acid procured by Luck[†] from the oil of *Madia sativa.*

[*] Phil. Mag. Ser. 3. vol. xxi. p. 161.
[†] *Annalen der Chemie und Pharmacie,* xxxv. 210.

The third acid is *margaric acid*. Heintz procured it by numerous recrystallizations of the portion of fatty acids chiefly containing it; it crystallized from alcohol in fine scale-like crystals, which solidified on fusion in shining, interwoven needles; its melting-point was exactly 140°.

	Free acid.				Baryta salt.	
	Found.		Calc.		Found.	Calc.
C^{34}	75·40	75·51	75·55	C^{34}	60·18	60·46
H^{34}	12·70	12·59	12·59	H^{33}	9·71	9·77
O^4	11·90	11·90	11·86	O^3	7·38	7·12
	100·00	100·00	100·00	BaO	22·73	22·65
					100·00	100·00

Lastly, the fourth acid is *palmitic acid*; it is the last precipitated by acetate of lead from the boiling solution of mixed acids, and appears to be contained in the greatest proportion in human fat. It melted exactly at 143°·6, and solidified on cooling into indistinctly crystallized, apparently laminar, shining masses, of a somewhat pearly lustre. When it has a small portion of margaric acid mixed with it, it crystallizes on gradual cooling after fusion in tufts of acicular crystals. From the alcoholic solution it crystallizes in small white scales.

	Free acid.				Silver salt.			Baryta salt.	
	Found.		Calc.		Found.	Calc.		Found.	Calc.
C^{32}	74·85	74·88	74·95	75·00	C^{32} 52·58	52·91	C^{32}	59·22	59·37
H^{32}	12·50	12·51	12·53	12·50	H^{31} 8·52	8·54	H^{31}	9·62	9·59
O^4	12·65	12·61	12·52	12·50	O^4 9·20	8·82	O^3	7·72	7·42
	100·00	100·00	100·00	100·00	Ag 29·70	29·73	BaO	23·44	23·62
					100·00	100·00		100·00	100·00

Dr. Heintz considers the olidic acid procured by Varrentrapp[*], by the action of hydrate of potash in a state of fusion upon oleic acid, to be identical with palmitic acid.

Dr. Heintz has also investigated the composition of the fluid portion of human fat. The oleate of baryta, prepared according to Gottlieb's[†] method, contained more baryta than accords with the formula given by that chemist; Heintz obtained from 22·2 to 22·5 per cent. of baryta, and a corresponding deficiency of carbon.

By repeated boilings of this oleate of baryta in so small a portion of alcohol that there was never more than a part of the salt dissolved at each operation, the residue contained at last as much as 22·7 per cent. Æther extracted from this impure oleate of baryta a salt which contained from 27 to 28 per cent. of baryta; the remaining pure oleate of baryta gave the formula proposed by Gottlieb.

	Found.	Calculated.
C^{36}	61·55	61·82
H^{33}	9·54	9·44
O^3	6·94	6·87
BaO	21·97	21·87
	100·00	100·00

[*] *Annalen der Chemie und Pharmacie*, liv. 124.　　　　[†] Ibid. lvii. 33.

The fluid portion of human fat consists therefore essentially of *oleine*, with which however a small quantity of some other fluid fat is incorporated, which is distinguished from the former in that the acid which it contains furnishes on saponification a baryta salt which is more difficult of solution in alcohol than the oleate of baryta, but on the other hand is more readily soluble in æther, and which contains much more baryta.

When human fat is exposed in the winter during a long period to a temperature about the freezing-point, the fluid fat separated from the solid parts allowed to stand until the next winter, and then again submitted for a long time to a similar low temperature, a considerable portion of solid fat will again separate; and the remaining fluid portion will again present the same phænomenon in the ensuing winter. This does not depend on a conversion of oleine into margarine; but Dr. Heintz found that this solid fat, purified by pressure and crystallization from alcohol, readily dissolved in a weak boiling solution of carbonate of soda. Thus, if human fat be left for a long time in loosely-stopped vessels, a gradual decomposition of the glycerine will occur and the fatty acids of the fat be set free; these are more difficult of solution in the fluid portion than the undecomposed fat, and occasion this repeated separation.—*Annalen der Chemie und Pharmacie,* lxxx. 297.

NEW ARRANGEMENT OF THE VOLTAIC PILE.
BY M. FABRE DE LAGRANGE.

I have found a means of rendering the current of the voltaic pile perfectly constant and invariable, even for weeks or months, of whatever metals the electrodes may be formed, and whether they be set in action by two liquids, as in the combination of Bunsen, or by one, as in that of Volta. This continuity of electric action is obtained in the same way that we obtain the continuity of the calorific action of a stove, which is furnished below with a grating to let the ashes fall, whilst we continually add fuel at the top.

The method which I employ is simple, and fulfills all the conditions which can render it practicable in an industrial point of view—instead of increasing the expense it diminishes it.

Let us first see the disposition of a single pair with one liquid. Take a vessel with a hole in the centre of the bottom, such as a flower-pot, and round the hole let one end of a cylindrical diaphragm of cloth be attached by cement to the bottom of the pot. The axis of the hollow cloth cylinder when erect will coincide with the axis of the vessel, and its height is somewhat less than the walls of the latter. Within the diaphragm is placed a stick of very hard coke, such as is found in the gas-retorts, surrounded by small grains of the same coke, and round the diaphragm a cylinder of amalgamated zinc and some acidulated water, furnished drop by drop from a reservoir above.

Let us now unite the two poles by a conducting wire, and see what takes place in the interior of the apparatus. The acidulated water, which continues to drop into the vessel, will pass in part over the margin of the cloth diaphragm on to the grains of coke, which

will thus be continually bathed by the movement of the liquid without being inundated, so that the polarization will be suspended and the bubbles of hydrogen will be freely disengaged through the interstices between the particles; besides which, the lower strata of the acidulated water, in consequence of the pressure which they have to support, will filter slowly through the cloth, which will not be the case to any extent with the upper and middle strata. Now these lower strata are precisely those which contain the sulphate of zinc which it is necessary to eliminate. The result is an electric current, which is perfectly constant until the entire disappearance of the zinc, and which is obtained with no more care than that of keeping the reservoir filled.

My method of uniting a number of pairs is as follows:—The stoneware pots in which they are contained, which are 3 or 4 diameters in length, and consequently have the appearance of tubes, are united and cemented into a bundle or block, which is readily transported from place to place. The upper surface being horizontal, small gutters are employed to convey the acidulated water to each pot. With this arrangement, by placing a second reservoir above the pile, and altering the nature and elevation of the diaphragms, it is easy to employ a second liquid, which may be made to fall directly drop by drop on the grains of coke, such as nitric acid;—it may be used with advantage when very weak, and when it will no longer serve for the battery of Bunsen from its ceasing to absorb hydrogen. The liquids on leaving the pots are collected and may continue to be used until saturation.—*Comptes Rendus*, April 5, 1852, p. 533.

ON THE PREPARATION OF PURE SILVER FROM CHLORIDE OF SILVER. BY C. BRUNNER.

It has long been known that pure silver for chemical purposes is best prepared by the decomposition of chloride of silver. This decomposition can be performed in various ways: Poggendorff[*] several years ago described a process in which it was effected by galvanism; this appears to me to be preferable to all others hitherto known, and the one here described can only be regarded as a modification of it.

Well-washed precipitated chloride of silver is to be put into a cup of silver, platina or copper, the outer surface of which is covered with wax, in such a manner that only a round space of one or two inches in diameter, according to the size of the cup, remains uncovered. On the bottom of a larger earthen cup a disc of amalgamated zinc is to be laid, on the middle of which the cup containing the chloride of silver is placed, in such a manner that the portion not covered with wax may come in contact with the zinc. Water slightly acidulated with sulphuric acid is now to be poured into the apparatus, until it rises above the margin of the inner cup, so that this will be completely sunk in the water. The decomposition of the chloride of silver immediately commences at the edge of the cup containing it, and proceeds inwards to the middle: this is readily known by the dark gray colour assumed by the silver as it separates; the decomposition will be completed in from 24 to 48 hours; its completion may be known

* Poggendorff's *Annalen*, vol. xxv. p. 342.

by there being no longer any chloride of silver visible on stirring the precipitate. The silver thus procured is to be washed with water, and any small residue of chloride of silver which it sometimes retains may be got rid of by diluted ammonia.

The silver thus prepared is perfectly pure. It is readily seen that any foreign metals that may be contained in the zinc, can never mix with it, as the disc of zinc lies during the whole operation below the cup containing the silver, and never comes in contact with it.—Poggendorff's *Annalen*, vol. lxxxv. p. 462.

THE BOMERANG. BY J. E. GRAY.

If a common manilla or palm-leaf hat having a low crown, and the margin of the rim sharply turned up about half an inch high, is thrown into the air with the cavity of the hat upwards, it returns back towards the thrower like the Australian bomerang. The angle at which it returns depends on the angle at which it is thrown; and if the angle is sufficiently acute, it will fall some distance behind the thrower.

The experiment depends on the position of the hat; for if thrown with the cavity downwards, it alights in the direction thrown, and does not return. A pasteboard disc with a turned-up edge has the same effect as a hat.

METEOROLOGICAL OBSERVATIONS FOR MAY 1852.

Chiswick.—May 1. Overcast: cloudy: clear. 2. Cloudy and cold: frosty at night. 3. White clouds: fine: clear and frosty. 4. Dusky clouds: clear and frosty. 5. Densely clouded: clear and frosty at night. 6. Cloudy: clear. 7. Overcast: very fine. 8. Cloudy: fine. 9. Fine. 10. Fine: rain at night. 11. Boisterous, with heavy shower, partly hail. 12. Heavy rain: thunder. 13. Cloudy: overcast: boisterous at night. 14. Showery and boisterous: clear. 15, 16. Very fine. 17. Slight drizzle: overcast: thunder, lightning and rain at night. 18. Very fine: rain at night. 19. Very fine. 20. Hazy: fine: showers. 21. Overcast. 22. Cloudy: clear. 23. Cloudy. 24, 25. Overcast: fine. 26. Rain. 27. Overcast. 28. Densely clouded. 29. Rain. 30. Fine: cloudy. 31. Fine: cloudy: clear and cold.

Mean temperature of the month	51°·45
Mean temperature of May 1851	51 ·16
Mean temperature of May for the last twenty-six years	54 ·07
Average amount of rain in May	1·74 inch.

Boston.—May 1. Cloudy. 2. Cloudy: rain A.M. 3, 4. Cloudy. 5. Cloudy: rain A.M. 6. Cloudy. 7. Fine. 8. Cloudy. 9, 10. Fine. 11. Cloudy. 12—14. Cloudy: rain A.M. 15, 16. Fine. 17. Cloudy. 18. Cloudy: rain A.M. and P.M. 19. Cloudy: rain A.M. 20. Fine. 21. Cloudy: rain A.M. and P.M. 22—25. Cloudy. 26. Cloudy: rain P.M. 27—29. Cloudy. 30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—May 1. Cloudy: fine. 2. Clear: fine. 3. Cloudy: fine: clear: fine. 4. Cloudy: fine. 5. Drops: fine: cloudy: fine. 6. Clear: fine: cloudy: fine. 7. Drops: rain: clear. 8. Drops: showers. 9. Rain: clear. 10. Drops: clear: aurora. 11. Cloudy: showers. 12. Bright: clear. 13. Rain: clear: fine. 14. Bright: showers. 15. Clear: cloudy. 16. Clear. 17. Clear: fine. 18, 19. Clear: fine: aurora. 20—23. Clear: fine. 24. Bright: fine: clear: fine. 25. Clear: fine. 26. Cloudy. 27. Cloudy: fine. 28. Bright: cloudy: showers. 29. Bright: showers: cloudy: hail-showers. 30. Sleet-showers. 31. Sleet-showers: showers.—This month has been fine, warm and dry.

Mean temperature of May for twenty-five years previous	47°·88
Mean temperature of this month	50 ·49
Average quantity of rain in May for six years	1·72 inch.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1852. May.	Barometer.					Thermometer.					Wind.				Rain.			
	Chiswick.		Orkney, Sandwick.		Boston.	Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.	9¼ a.m.	8½ p.m.	8½ a.m.	Max.	Min.	8½ a.m.	9¼ a.m.	8½ p.m.	1 p.m.							
1.	29·873	29·729	29·85	29·98	29·33	57	34	47·5	50	46	n.	n.	n.		·01			
2.	30·082	29·980	30·08	30·15	29·62	51	25	44	50	47	ne.	n.	nw.			·04		
3.	30·171	30·101	30·22	30·29	29·70	57	28	47	50	45	ne.	n.	......					
4.	30·190	30·162	30·28	30·29	29·80	56	27	48	51	51	ne.	n.	whnw.			·02		
5.	30·240	30·216	30·27	29·90	29·78	59	30	50	55	52	ne.	n.	wsw.					
6.	30·239	30·162	30·14	29·82	29·76	54	34	51	55	55	ne.	nw.	wsw.					
7.	30·106	30·040	29·85	29·57	29·60	70	38	55	50½	48	w.	w.	se.				·20	
8.	30·061	30·043	29·67	29·70	29·50	69	43	63	53½	51	w.	w.	wsw.				·10	
9.	30·055	30·026	29·48	29·48	29·46	73	40	57	54	50½	sw.	ssw.	sw.		·05			
☽ 10.	30·026	29·852	29·61	29·20	29·34	60	43	58	52	47½	sw.	sw.	se.		·09			·02
11.	29·945	29·831	29·12	29·16	29·27	61	46	55	51	47	sw.	s.	sw.		·25			
12.	29·883	29·639	29·34	29·40	29·13	67	49	55	47	48	sw.	w.	wsw.		·02	·04		·23
13.	29·724	29·680	29·42	29·43	29·18	59	51	61·5	49	46	sw.	sw.	h.		·02			·15
14.	29·714	29·586	29·40	29·48	28·99	62	38	54	50	50½	sw.	w.	w.					
15.	29·953	29·935	29·76	29·60	29·58	65	39	55	50	50	sw.	w.	sse.			·04		
16.	30·077	29·811	29·55	29·46	29·30	74	47	59	51½	51	sw.	s.	sw.		·33		·12	
17.	29·825	29·719	29·53	29·63	29·30	66	47	64	54	49½	sw.	calm	ne.		·11			
18.	29·851	29·569	29·89	30·05	29·33	72	45	61	54	50	sw.	ne.	n.			·02		
● 19.	29·662	29·785	30·06	30·02	29·30	72	43	60	53½	49½	se.	wsw.	e.		·10	·16		
20.	29·937	29·971	30·09	30·14	29·50	67	48	56	55	49	se.	ene.	n.					
21.	29·985	29·972	30·14	30·15	29·72	61	46	50	53	48½	n.	nne.	e.			·06		
22.	29·992	29·995	30·24	30·28	29·56	61	47	51	56	53	ne.	nne.	ne.					
23.	30·039	30·027	30·26	30·21	29·60	57	45	52	52	46	ne.	ne.	e.					
24.	30·062	29·876	30·17	30·18	29·64	66	49	54	57	48	ne.	nnw.	e.					
25.	29·955	29·790	30·18	30·21	29·55	61	49	52·5	58	48	ne.	nne.	......		·11			
☽ 26.	29·850	29·875	30·16	30·18	29·48	54	49	50·5	56	51	ne.	ne.	n.		·35			
27.	29·902	29·779	29·98	30·08	29·40	54	47	50	47	45	ne.	n.	......		·01	·07		
28.	29·844	29·611	29·98	29·93	29·30	57	46	52	52	45	ne.	nne.	n.		·03			
29.	29·689	29·686	29·80	29·73	29·26	51	35	52	52	45½	nw.	nw.	nw.		·25		·12	
30.	29·763	29·795	29·59	29·60	29·33	57	35	48	49	45½	w.	nw.	nw.		·01		·35	
31.	29·847		29·61	29·66		60	34	51	47	46		nnw.	nw.				·13	
Mean.	29·953	29·845	29·868	29·861	29·44	61·61	41·29	53·7	52·35	48·64					1·74	0·45	1·42	

XI. *Renewed Inquiries concerning the Spiral Structure of Muscle, with Observations on the Muscularity of Cilia. By* MARTIN BARRY, *M.D., F.R.S.**

[With Two Plates.]

[THESE renewed examinations of muscle, extending over a period of many months, were made in the house of the veteran Purkinje; whose judgement in the matter may be inferred, when it is stated that,—after what he had seen during that long period of almost daily demonstrations by the author, and the devotion of whole mornings to the subject with almost every kind of muscle that could be obtained,—he translated an account of the same into German, printing no fewer than sixty-eight pages.]

Deprived for years of the pleasure of microscopic research by an affection of his sight, the author finds himself in this respect so far restored, that it does not appear necessary to deny himself that pleasure any longer. If he has erred in taking up the microscope again, that is to say, should his sight be thereby injured, he thinks he may well claim to be excused.

For it could not be to him a matter of indifference that his researches on the structure of muscle had not met with the attention he had expected, and which the subject well deserves. He therefore felt driven to renewed research; and, after nine months of still more rigid investigation, does not find occasion to give up his former views. So far from this, indeed, his opinion that muscle has no other than a spiral structure has been even more confirmed. He has met with unexpected states

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* Communicated by the Author; being the substance of a paper translated into German by Professor Purkinje, Foreign Member R.S., and by him communicated to Müller's *Archiv*, Heft vi. 1850.

of interlacement, throwing new light upon the whole ; and now understands how it was that observers did not see what he has seen.   The attention of some at least appears to have been given so exclusively to one or to another of those states of interlacement, that they remained, as it were, at different stages in their attempts at explanation.   Probably his double spiral fibril appeared to most observers too complicated, perhaps too artificial. Now, however, having found and figured transition stages extending to complete relaxation, he hopes that there will be some at least disposed to repeat their examinations, and with more minuteness than before.   Then, perhaps, justice may be done to the author's views, instead of having assigned to them the mournful honour of figuring somewhere in history as opinions or even errors. The objects are of extreme minuteness, requiring almost without exception the highest magnifying powers ; and they are optically so complicated, that nowhere in the field of microscopic observation is there a subject more difficult than that of muscle.

The principal facts made known by the author in the Philosophical Transactions of the Royal Society for 1842, briefly recapitulated, were these :—The muscular fibril consists of two spiral threads, so interlaced as to present a double cylinder, *i. e.* the fibril appears as if grooved on each side, a transverse section of the fibril being represented by the figure (∞).   The fibril of a primitive fasciculus having transverse striæ is usually so situated that one of its edges is directed towards the eye of the observer ; whence it comes that usually there is seen the spiral of only one of the cylinders of the fibril.   A muscle is thus nothing less than a vast bundle of spirals, appearing short and thick in contraction, long and thin in relaxation.   The elliptical winds of the spirals appear to have been mistaken by some observers for " beads," " segments," or " particles." The dark *longitudinal* striæ of the primitive fasciculus are spaces (probably occupied by a lubricating fluid) between the edges of the fibrils.   The dark *transverse* striæ are rows of spaces between the winds of the spiral threads constituting the fibrils.   If the dark longitudinal striæ are spaces between the edges of the fibrils, the light longitudinal striæ are the edges themselves of the fibrils ; and if the dark transverse striæ are rows of spaces between the curves of spiral threads, the light transverse striæ are of course the visible portions themselves of those spiral threads.   The contraction of muscle does not require a flattening of " segments " or " particles," as supposed by some, but simply a more transverse direction of the spirals in their curves.   Hence in contraction the striæ of a fasciculus are narrow, and in relaxation they are broad, denoting a shortening and lengthening respectively of

the fibrils. "Transverse cleavage" of the primitive fasciculus is caused by the spirals giving way at that part where they offer the least resistance, viz. at the part where they cross one another and are in contact. The sarcolemma arises from the coalescence of spirals.

It was by attending to the history of development of muscle, chiefly in the larva of the large toad of Jersey, that the author arrived at a knowledge of the foregoing facts. His observations published in the present paper were made with one of the large compound microscopes of Plössl; and it happened that the instrument was the very last constructed by that justly renowned optician. The following are some of the new facts herewith observed; others will be mentioned further on.

The two spirals of which the muscular fibril consists, run in the same direction, and not in opposite directions, as the author at first supposed. (Plate I. fig. 1.)

These two spirals are united at the end of the fibril, as in a loop. Such at least is the case in one form of muscle, Pl. II. fig. 30, and from analogy it may be presumed to be the same in others.

The fibril, being thus a double or twin spiral, undergoes a stronger twisting in contraction, and an untwisting in relaxation.

When met with in relaxation, the two spirals usually present themselves in a state comparable to that of common twine. (Pl. I. fig. 2.)

Between the untwisting in relaxation and the twisting in contraction, there are several intermediate states. Fig. 3 presents four such intermediate states seen in different parts of the same fibril.

Cilia also are double spiral threads, and thus have a structure like that of the muscular fibril. (Figs. 29, 30, 31, &c.)

The author then describes models of lead wire which he has constructed for the purpose of illustrating the structure of the muscular fibril, fig. 1. These models, though very rude ones, may afford some idea of the different conditions of the fibril in regard to length, breadth, and thickness, in different degrees of contraction, *a*, and relaxation, *b*. (*b*, in the model, fig. 1, represents part of *a*, after the drawing of the latter out*.)

It is an error to suppose it possible to learn how the striæ in muscle are produced, by examining merely the primitive fasciculus. The primitive fasciculus must be, as far as possible, teased out with needles, and the fibrillæ separately examined. To obtain the fibril, muscle should be selected in which the primitive fasciculi are small. For this purpose the heart is especially to be

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* [Such models have since been presented to the Royal Society and to the Royal College of Surgeons in London by the Author.—May 1852.]

recommended; because, from the ever-active condition of that organ, its muscle is in a state which he regards as that of continued renewal; and the fibrils are not so very difficult to separate from one another. In no heart that the author has examined are the fibrils seen with more distinctness to be double spirals than in the heart of the common frog. He has frequently found such states of the fibril as those in fig. 2, and fig. 3 *f*, to be seen with remarkable distinctness in muscle from the tail of the Crawfish, *Astacus fluviatilis*. The observer should use for examination the muscle of a healthy animal just killed, and never employ muscle that has been preserved in spirit or in any other fluid. The least degree of decomposition is sufficient to destroy the spirals. Decomposing muscle presents granulated threads enough (fig. 6 *b*), *i. e.* rows of cell-germs, but rarely any trace of spirals. (From this it is seen, and it is important to observe, that the spiral threads are more easy of decomposition, and disappear sooner than the formative cell-germs.)

The accustomed eye can often discern spirals without a chemical reagent; but for those unacquainted with them a reagent is essential. The author provides two solutions; the first a solution containing $\frac{1}{200}$dth of corrosive sublimate in spirit of 0·940 spec. grav., and the second a concentrated solution of corrosive sublimate in distilled water. Having placed a drop of the first of these solutions upon glass, he introduces into it a minute portion of muscle from the heart of the frog, and *immediately* proceeds to carefully spread it out with needles. He then separates longitudinally a portion as minute as possible from the rest, and teases this out in the same drop under a lens *to the very utmost*. The threads thus prepared he removes to another strip of glass upon which he had placed a drop of the solution No. 2, covers them with a bit of the thinnest glass, and views them under the microscope,—at first with a power of about 200 diameters, in order to select the threads best adapted for observation, and bring them into the middle of the field of view. He then applies a power of about 600, and usually finds here and there a fibril sufficiently separated from the rest at the edge of the preparation to enable him to discern its spirals. It is not unimportant to remark, that the muscle must always be first teased out in the solution No. 1, before it is introduced into the solution No. 2; for, besides corroding the needles, and making them quite useless, the solution No. 2 instantly renders the substance of the muscle so compact, that teasing of it out is absolutely impossible. Even the solution No. 1 lays hold of the muscle to a certain extent, so that the observer should proceed as soon as possible to apply the needles. If it be desired to make preparations that are to be preserved for any length of time, the concentrated aqueous solu-

tion of corrosive sublimate (solution No. 2) of course cannot be
employed, as the fibrils become speedily decomposed and indi-
stinct. For such preparations the author prefers a solution
containing $\frac{1}{120}$th of nitrate of silver in distilled water, using this
instead of the solution No. 2, but having proceeded as before
with the teasing out in solution No. 1. It is true that the ni-
trate of silver does not show the spirals with the same remarkable
distinctness as the solution No. 2, and besides, the preparation
is here and there defaced with a precipitate; but for prepara-
tions to be preserved for any length of time, it is to be preferred.
For immediate examination the author especially recommends
the solution No. 2.

The use of chemical reagents having been objected to, it may
be replied, as suggested to the author by his brother, J. T. Barry,
were anyone *denying* the existence of the structure in question,
then it might be very proper to object, that reagents had de-
stroyed it; but when the existence of that structure is *affirmed*,
it cannot be objected that that structure has arisen through those
reagents, especially when, in order to bring it into view, sub-
stances so very different have been used, as corrosive sublimate,
nitrate of silver, and chromic acid. Least of all can those object
to the use of chemical reagents who in such researches employ
maceration, which, as is known, does not require much time to
produce in organic substance the greatest changes.

Adhering to his original views regarding the situations of the
striæ in the fasciculus of muscle, as above quoted, the author
gives figures illustrative of the same (figs. 4 and 5). These
show the situations of the dark longitudinal striæ to correspond
to the spaces between the edges of the fibrils, and the situations
of the dark transverse striæ to correspond to the crossing places
of the winds of the spirals. It is obvious from the same figures
that both the longitudinal and transverse striæ are produced by
the refraction of light; for at the very part where the dark striæ
present themselves, the rays from the mirror of the microscope
fall upon oblique surfaces, where they are diverted from their
direct course and do not reach the eye. The dark longitudinal
striæ are produced by the cylindrical form of the elementary
muscular threads, and the dark transverse striæ arise partly from
the same cylindrical form of the muscular threads, but chiefly
from the oblique direction of the same at their crossing.

Had observers paid due attention to the history of develop-
ment, they could not have failed to observe a pellucid gelatinous
substance to which the author has given a name suggested to
him by Professor Owen, that of hyaline; a name descriptive of
the appearance only, though the substance evidently performs

functions rendering it in importance second to none.   In muscle, this substance, hyaline, is often found within the winds of the spiral threads; often the fibril is enclosed within a cylinder of hyaline, fig. 3.   It is very important to be aware of the little difference in refractive power between the hyaline and the substance of the spirals, whereby the outline of the latter becomes almost invisible.   This is especially the case when the fibril still lies in the primitive fasciculus, and even occurs after its separation from it.   Hence the different views taken by observers of the fibril, especially that assigning to it a structure comparable to a row of varicosities or beads.   It is therefore equally important to apply reagents that will serve to introduce a greater difference in the refractive power of the two substances in question, and thus diminish the misleading influence of the hyaline.

This hyaline appears in another way to have misled observers. Where contained within the winds of spiral threads, fig. 6 *a*, it holds together a row of cell-germs; which cell-germs, on the wearing out as contractors of the old spiral threads, give the material for new ones.   And some observers, overlooking the spiral threads, probably mistook such rows of cell-germs for fibrils.   This mistake is very likely to be made when the muscle has undergone a slight degree of decomposition, fig. 6 *b*, whereby the spiral threads dissolve and disappear sooner, leaving exposed the axis of hyaline with its row of cell-germs.   Prof. Bowman appears to have figured such an axis of hyaline containing cell-germs, as a fibril*.   It may appear absurd to suppose that any doubt on such a matter can be entertained; and yet, since the mistake in question has been made, the author ventures to ask physiologists which appears to them the more probable: that spirals are formed first in order to produce cell-germs (!), or that cell-germs are first formed in order that they may give origin to spirals ?   *c* and *d* in fig. 6, show division and subdivision of the cell-germs for the production of minuter spirals.   It must be admitted that the changes in the structure of the fibrils, attending their continued composition or decomposition, present a series of transition states such as may mislead all engaged in this most difficult field of observation.

It is known that in some states the primitive fasciculi during manipulation break off short, that is transversely, and that in other states they divide in a longitudinal direction; but it is not known on what this difference depends.   The author explains it thus :—He finds the tendency to transverse cleavage to be in proportion to the amount of contraction the muscle happens to

* Cyclopædia of Anatomy and Physiology, article "Muscle and Muscular Contraction," fig. 287 *c*.   In his earlier work, Phil. Trans. 1840, no such figure is to be found.

be in at the time of manipulation, while relaxation in the same proportion causes the giving way in a longitudinal direction; and he offers the following as perhaps sufficient to account for the difference. In contraction, where the transverse striæ are narrow, the spirals cross each other (*i. e.* antagonize each other) at the acutest angles; and in such a state of course it is that there occurs most easily a mutual *cutting through,* producing the " discs " of Bowman, to be again referred to. On the other hand, in relaxation the spirals meet only at obtuse angles, whereby the tendency to cutting through is in proportion lessened. The cutting through of the spirals when crossing at acute angles is illustrated by reference to a play with twine, familiar to school-boys.

In the early stages of development, however, fasciculi are sometimes met with in which the fibrillæ are so surrounded with large spirals, that longitudinal cleavage would be difficult however complete the relaxation. Of such large spirals, not merely two, but many interlace,—each surrounding its own cluster of fibrils. These large spirals pass into membrane and form septa*. Now within the winds of the larger spirals there arise smaller ones, which in their turn enlarge and pass into membrane, to be succeeded by another generation, and so on; by which it is easy to understand the prevention of longitudinal cleavage, as well indeed as the difficulty constantly met with when endeavouring to obtain separate fibrils for microscopic examination. Again, the state of the primitive fasciculus in fig. 7 was met with; where the fibrillæ, *c*, were shared by more than one surrounding spiral, *b*; the whole being surrounded by a larger spiral, *a.* Here also cleavage in a longitudinal direction would be very difficult. Further, the author saw states in which there was absolutely no cleavage, the fasciculus before breaking off becoming tapered to a point, fig. 8. This tapering to a point seemed referrible partly to great distensibility of the sarcolemma, and partly to a loose condition of fibrils already somewhat relaxed; and it is beautifully demonstrative of a spiral structure. (See the figure, and contrast the direction of the curves of the spirals at *a* with that at *b*.) Besides, at *a* the fasciculus was thick, while at *b* it was thin; and as the spirals became more and more drawn out, the fasciculus became more and more thin, until it terminated in a point. (The arrow shows the longitudinal direction of the fasciculus.)

It not rarely happens in the breaking off of twine, in which the two threads composing it are of unequal extensibility, that

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* Bowman observed that the inner surface of the sarcolemma often presented irregularities, which the author thinks were no other than remains of septa such as those above mentioned.

one of them is more drawn out than the other, which becomes coiled around it as around an axis. Such a state being not unfrequently presented by twine-like muscular fibrils, fig. 9, after the breaking of them up with needles, it is important that the observer should be aware how the appearance is produced; for it may easily mislead him into the belief that he sees a row of alternately longer and shorter "beads."

The author is convinced that, with the exception of one case already mentioned (fig. 6 *b*), in all instances where Prof. Bowman speaks of fibrils, he had before him, without recognising them, nothing less than spirals. "Very reluctantly," says the author, "should I again enter into a controversy with a fellow-countryman whom I much esteem, were I not sure that his desire to arrive at the truth in this matter is quite equal to my own." He then gives copies of five of Bowman's figures, fig. 10 *a*, *b*, *c*, *d*, *e*, placing beside them five corresponding figures of his own, fig. 11 *a*, *b*, *c*, *d*, *e*, and showing the former to be, not, as supposed by Bowman, rows of beads, but different states of double spirals. No doubt, it is added, Bowman's fibrils had undergone some change; for three out of five of the preparations from which they were drawn had been preserved in spirit, while the fourth had been exposed to maceration.

What the author states of Bowman's figures of fibrils applies equally to the drawings given by that physiologist of fasciculi, though the latter are on a smaller scale. And no one, he thinks, who will take the trouble carefully to compare Bowman's figures 39 and 40, in his memoir, Phil. Trans. 1840, as well as those in his (Bowman's) Plate 19, in the same memoir, with what has been said in the present paper of the change in breadth of the transverse striæ in consequence of the difference in direction of the winds of the crossing spirals, will refuse to admit that the latter serves fully to explain the former.

We are indebted to Bowman for representations of manifold appearances presented by primitive fasciculi during their contraction and expansion, though from being unacquainted with the spiral structure of muscle he could not explain them, and wisely avoided the attempt to do so, except that he sought to refer the approach towards, and withdrawal from one another of the transverse striæ, to contraction and expansion of his supposed "discs."

But what are these "discs" of Bowman? Certainly not what he thinks, layers of muscular substance, "primitive component particles," an assemblage of which constitutes the primitive fasciculus. Bowman's discs are really nothing else than the bright parts of the transverse striæ, in which the single winds of the spiral threads are arranged in adjacent order (fig. 5 *a*, *a*, *a*), and

as it were, into ' *étages* ' or series. (See fig. 12, where one of these
' *étages*,' the lowest, is separated from the rest by the cutting
of the spirals at their points of crossing.)   The dark places in
the transverse striæ correspond to those separating Bowman's
discs.   They are nothing else than the crossing places of the
spiral threads.   Here the latter come into immediate contact with
one another,—can with pressure be made to exercise a cutting
power,—and, as before said, actually to cut each other through.
This, too, must take place more or less in stories or ' *étages*,' as
the points of crossing are for the most part on the same level.
And when the cutting through has taken place, each story or
' *étage* ' represents one of Bowman's discs*.   (The author here
points out a difference between merely perspective crossing of
the spirals, and that crossing where they are in contact ; it being
of course at the latter only that there can be a cutting through.)

Whence comes it that, as was observed by Bowman, contrac-
tion at any part of the primitive fasciculus (characterized as this
is by greater nearness of the transverse striæ) is attended, both
before and behind that part, in the longitudinal direction, with
a separation of the transverse striæ ?   The cause, according to
the author, is simply this : when the spiral threads extend more
in a transverse direction at one part than at another, this can
take place in no other way than at the expense in the longitu-
dinal direction of their continuations, the winds or loops of
which, thereby drawn out of the transverse direction, assume one
that is more longitudinal.

Bowman is right in maintaining that contraction of the primi-
tive fasciculus has nothing to do with zigzag inflexions of the
same.   On the contrary, as Bowman remarks, it has been shown
by Owen that it is in *relaxation* that these zigzag inflexions may
arise ; and not only so, but that in the Filaria they are regularly
present in relaxation, being there indeed characteristic of the
relaxed state of muscle.   The author inquires, How then does it
happen that such zigzag inflexions may arise in relaxation ?   He
thinks it may possibly be in the following manner :—Suppose
the extreme ends of the primitive fasciculus through any hin-
derance to remain fixed, and that the fibrillæ, after cessation of the
influence of the contractile force, strive by means of their own
elasticity, and in consequence of the relaxation of their spiral

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* This, however, is not always the case ; for at different parts in the
breadth of the same primitive fasciculus the fibrils may be in different de-
grees of contraction, and their points of crossing therefore on different
levels.   In such states the transverse striæ, viewed with changes of focal
distance, are seen to change their place continually, according as viewed
near the periphery or at greater depth, as was observed by Bowman, and
as every experienced microscopic observer must have noticed.

threads, to gain a greater length; but being prevented from doing so by the hinderance above supposed at their ends, they seek to gain that greater length through lateral inflexions, which in such a case must produce a zigzag form. Were an antagonizing force applied, the elongation could follow without the formation of such zigzags.

We are indebted to Prof. Bowman for many microscopic measurements of the primitive fasciculi in different classes of animals. He found the largest in Fishes; they had a less size in the Amphibia, were smaller in Mammals, and smallest in Birds. Bowman's measurements are very numerous, and were no doubt made with the greatest care. He has, however, omitted to draw general conclusions therefrom, and makes no remark as to the cause of those differences in size. The author in this respect follows the example of Prof. Bowman. He brings forward no general conclusions of his own on the subject, and indeed for this reason: because he thinks that we ought first to have determined the mean size of the primitive fasciculi in the same individual as well as in different individuals of the same species, according to their different manifestations of activity, before we undertake to draw general conclusions. Yet he cannot refrain from here pointing to the following fact, mentioned in a former part of his memoir. According to his observations, the primitive fasciculi are at first merely double spirals, *i. e.* they are no other than fibrils. The metamorphosis of fibrils into primitive fasciculi is especially observable in the heart, where the young fasciculi are found, at first flat and scarcely broader than the fibrils themselves. The cause of these continued changes in the muscle of the heart, as already said, it may well be supposed is no other than the ceaseless activity of that organ. Scarcely is it to be doubted that the same thing takes place in other muscle also, though more slowly. (Certain muscles are then referred to as affording examples of great activity, and it is remarked that in different individuals of the human race the primitive fasciculi in the muscle of certain parts are probably unusually small.) Hence it occurred to the author to offer the following remarks concerning the measurements of Bowman. In Fishes the primitive fasciculi were found the largest, because of the low degree of muscular activity required in the element in which they live (though perhaps the muscles of the fins and gills may be here excepted, and it is known that they present even externally an appearance different from that of the other muscles). In Birds, on the contrary, the primitive fasciculi were found the smallest, as was to be expected from the high degree of muscular activity of this class. Amphibia and Mammals presented a middle average size, from their muscular activity occu-

pying a middle place; but here it must be remarked, that striking differences would certainly have been met with had Bowman's researches been of a more special kind; for it is probable that the salamanders and other naked Amphibia would have presented still larger fasciculi than even Fishes.

Schwann was the first to make us acquainted with the existence of the sarcolemma. An independent discovery of it was made by Bowman, to whom it is that we are indebted for an exact description of that structure and an appropriate name. Its mode of formation out of spirally arranged cells, the author believes to have been first seen and published by himself. That mode of formation of the sarcolemma appears to be as follows:—At *a*, fig. 13, is a coil of young cells (once a column of compound cytoblast, as at fig. 20 *c*). *a*, in fig. 13, is a drawing from nature; *b* in the same figure is a diagram. *a* passes into *b*. That the spirals really do consist of cells is seen from fig. 14, a drawing from nature, in which *a* represents a large double spiral from the tail of the tadpole when very young, and *b* the remains of a similar double spiral after the addition of acetic acid. The acid removed the coalesced membranes of the cells, of which the double spiral *a* was composed, and left the nuclei behind in double spiral order. At *c* is seen the structure of one of those nuclei. They contained the elements of division, by which division the spirals pass into the state of membrane. And fig. 15, also from nature, shows such division to have taken place; this figure representing a stage in the formation of the sarcolemma.

With regard to the function performed by the sarcolemma, no definite opinion appears to have been given. The author believes that its function depends on elasticity. As the walls of the sarcolemma-cylinder are distended during the contraction of the double spiral threads, they return inwards as soon as relaxation comes on. And it is in this manner that the active relaxation of the fasciculus of muscle is to be explained.

Muscle from the thigh of the Grasshopper (of which many individuals, including several species, were examined) having uniformly presented a relaxed state nearly approaching to that in fig. 16 (which, however, is merely a diagram), it appeared to the author probable that such a state was not unconnected with the sudden muscular contractions required by this creature for its leaps. A sudden change from such a state of relaxation to that of extreme contraction must here take place with the greatest facility, and be combined with the manifestation of great power. This opinion having been mentioned to Prof. Purkinje, the latter recommended the author to examine the corresponding

muscle from the Flea, in which, from its enormous leaps, some-
thing similar would be thought be found.   The author accord-
ingly examined some of these, and had the satisfaction to find in
them a degree of muscular relaxation even higher than that he
had observed in the grasshopper.   In the two figures, fig. 16 and
fig. 5, the parts in fig. 16 marked *b, b*, correspond to *b', b'* in
fig. 5.   From a comparison of these two figures, it will be at
once seen how the extended *b, b* in fig. 16, pass in contraction
into the narrower *b', b'*, fig. 5.   Similar conditions no doubt
exist in other animals, but perhaps nowhere are they more re-
markable and constant than in those just mentioned.   The
observation may possibly induce some to bestow their attention
upon this subject when examining leaping insects as well as
other animals.

The author repeats a drawing he gave in the Phil. Trans. for
1842, of an artery from the pia mater of the Rabbit, fig. 17, of
which the following is an explanation :—*a*, longitudinal muscular
fibrils, represented merely by rows of dots, except a single one
on the left side in which is shown the double spiral; *b*, outline
of a fibril surrounding the longitudinal ones; *c*, double spiral
structure of *b*; *d*, blood-corpuscles, for the most part young and
very small; *e*, a line denoting the inner membrane of the artery.
He then gives a figure, fig. 18, representing more distinctly the
double spiral structure of such a fibril as *b* in fig. 17.

His observations on the history of development of muscle are
given in detail, with many illustrative drawings; but as only a
part of the latter can be given in this abstract, it is not intended
to offer here more than the substance of the principal facts he
observed, which were as follows :—
Cells having arranged themselves as at *a*, fig. 19, and their
membranes having passed through the states *b* and *c* in the same
figure, and a tube having been thus formed (stages known to
other observers), columns of compound cytoblasts are seen within
the tube, fig. 20 *b, c*; which cytoblasts have descended by divi-
sion from the nuclei of the primitive cells, fig. 19 *a*. (The com-
pound cytoblasts in these columns are arranged with such regu-
larity as to produce, and explain the nature of, the striæ seen by
Schwann, fig. 20 *a*.)   The membrane of the tube disappears, not
forming, as Schwann thought, a permanent sarcolemma; and
the columns of compound cytoblasts having passed into coils of
cells, fig. 13 *a*, a spiral is formed of them, as shown by the dia-
gram *b* in the latter figure.   A central row of cell-germs is left
for the formation of future spirals; and the spiral first formed
divides, and, as above shown, passes into membrane—the first

sarcolemma. Such future spirals in a far later stage are seen in fig. 21; and fig. 22, *a, b, c*, shows the way in which the cell-germs perpetuate themselves by division and subdivision, every spiral having within its winds the elements of reproduction, fig. 23; and the primitive fasciculus being often found to have preserved cell-germs for a more general purpose in a central line, fig. 21. The reproduction of muscle, when fully formed, is probably no other than a continuation of its history of development, and has been already illustrated in fig. 6. By self-division of its hyaline axis of cell-germs, every fibril may become converted into a primitive fasciculus.

The laws of development in general are best studied in the ovum; and he who holds the wondrous process of cell-formation in the germinal vesicle, *i. e.* the history of development of the germinal spot described by the author in the Philosophical Transactions for 1840 as undeserving of particular attention, may spare himself the trouble of inquiring into the history of development of muscle, or that of any other tissue, as his labour would be thrown away. In that development of the germinal spot, the hyaline in the centre of the spot is obviously the prime mover. It is the hyaline in the centre of the germinal spot that is the substance undergoing fecundation; and no doubt it is the hyaline seen in the head-like extremity of the spermatozoon that is the real fecundating substance. (The author once saw, and figured in the Philosophical Transactions for 1840, what appeared to him to be a spermatozoon in the very act of entering the ovum of the rabbit; its head having already penetrated an orifice discernible for a time in the zona pellucida*.) In the

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* He mentions having repeatedly found unaltered spermatozoa in the interior of the ovum in its next stages, after it had passed into the Fallopian tube; and having had the opportunity of showing them to Professor Owen, who declared himself fully convinced of the presence of the spermatozoa within the ovum. Once the author counted as many as seven in a single ovum. (A drawing of that ovum will be found in a paper by him "On Fissiparous Generation," in the Edinburgh New Philosophical Journal, October 1843.) In all instances the spermatozoa were motionless; and not among the cells in which the development of the essential substance was proceeding, but in the colourless fluid between those cells and in the zona pellucida. [While passing through London in May 1852, the author learns that after the lapse of many years these observations have been in two quarters confirmed by others; Dr. Nelson having presented to the Royal Society a paper announcing the presence of spermatozoa in the interior of the ovum of a creature at the other end of the animal kingdom, *Ascaris mystax*; and Mr. Newport having added a postscript to a paper of his on the ovum of the frog, also presented to the Royal Society, in which he candidly acknowledges having erred when, in a former memoir, he questioned the accuracy of the discovery made by the author of the present paper, that entire spermatozoa do actually make their way into the interior of the ovum.]

ovum of the Rabbit, after fecundation, the germinal vesicle returns to the centre of the ovum, and the fecundated hyaline passes to the centre of the germinal vesicle. This hyaline, in consequence of fecundation, now contains substances of two kinds,—one from the female ovum, the other from the male fecundating fluid. Through a process operating in the germinal vesicle before fecundation, the hyaline of the ovum had prepared a sort of pabulum,— minute globules of hyaline. With this pabulum, the new hyaline, a compound of male and female elements, proceeds to nourish itself; or, in other words, proceeds to assimilate the contents of the germinal vesicle, whereby there arises a material for the formation of two cell-germs into which it divides. These two cell-germs grow at the expense of the remaining contents of the germinal vesicle, which are nutrimental cells, until the whole are consumed. The membrane of the germinal vesicle, the mother-cell of the whole body, has now disappeared, and there are seen in the place of that vesicle two young cells, which together constitute the new organic being. How shall we designate the hyaline of this new being? If we call the hyaline of the ovum, hyaline No. 1, and that from the fecundating substance, hyaline No. 2, we have in the new organic being, hyaline No. 3. No. 1 denotes the maternal hyaline, No. 2 the paternal fecundating substance, and No. 3 composed of the first and second, the hyaline of the offspring. Hence it is that the offspring comes to resemble both parents; for, be the resemblance effected as it may, the so compounded hyaline of the offspring will never lose a constitution inherited partly from the father and partly from the mother. And how does the hyaline of the offspring now begin to propagate itself, so that at last a creature shall arise out of it, in stature and other peculiarities like the parents? This is effected by self-division and repeated self-division. Each of the two cells just mentioned, together constituting the new organic being, becomes in its turn a mother-cell, so that now there are four; and in like manner there arise 8, 16, and so on, until the whole assumes the appearance of a mulberry. In the centre of this mulberry-like aggregate of cells there now appears one larger than the rest, like a queen-bee in the hive. This is the only cell in the group that has an enduring existence, *i. e.* in its progeny; all the others serve but a temporary purpose. (We thus have a sort of aristocracy of cells! first manifesting itself in the two above-mentioned as arising in the germinal vesicle, and nourished at the expense of all the surrounding cells.) This large cell now moves from the centre of the ovum towards the periphery, and here takes a fixed station. The hyaline nucleus of this cell is now to be considered as the most peculiar germ of the whole organism. Out of the nucleus of this cell, after many intermediate stages of formation, there at length arises the " pri-

mitive trace," and Von Baer's "chorda dorsalis." For other
details, the author refers to his researches published in the Phi-
losophical Transactions for 1839 and 1840; not deeming it
suitable to the purpose of the present paper to add more, than that
the process through which the first and continually repeated self-
division of the hyaline is effected, is no other than a repetition
of the same process which operates in the germinal spot of the
germinal vesicle, as the original cell of the organism; in which
process the operation of certain functions required for an increase
of substance is implied, viz. absorption, assimilation, and secre-
tion. In the cells thus descending from the original mother-
cell down to the remotest generation, it is evident that the same
wondrous process is repeated, the same increase of the hyaline;
which at first takes a peripheral station in the cell in order
through absorption to be newly fecundated (for what in this
case is absorption, but the fecundation of the hyaline of the cell
through a relatively external substance maintaining the process of
division?) Then, after fecundation at the periphery, the hyaline
passes into the middle of the cell, there again to divide into new
generations of cells, which finally arrange themselves so as to
form the various tissues of the organism. But the germinal
spot process continues even here. (Compare the contents of the
cell in fig. 19 *a*, with the author's delineations of the contents of
the germinal vesicle, Phil. Trans. 1840, Plate 22. figs. 159,
160, 162 *c*.)

Every one who has noticed the author's drawings of a certain state
of the two cells succeeding the germinal vesicle, must have been
struck with the resemblance they bear to corpuscles of the blood.
He deems it important in this place to refer to observations he
long since published, that both have the same destination;
through both these structures, as well the blood-corpuscles as
the cells of the ovum, is it intended to reproduce the hyaline,—
the one being floating, and the other fixed centres of that pro-
cess of assimilation which effects the reproduction of the hyaline.
The germinal vesicle may be regarded as a living being; and
every blood-corpuscle as one of the progeny of the germinal
vesicle, reproducing itself, as that vesicle itself does, by division
of its fecundated hyaline. We may consider the blood-corpuscles
as a floating shoal of Infusoria, receiving as their nourishment
the chyle. So nourished, or rather (as regards their hyaline
centres) so fecundated, the blood-corpuscles repeat in their in-
terior the whole germinal spot process, since in some of them
there proceeds the self-division and repeated self-division of the
hyaline, whereby new generations of blood-corpuscles arise, which
again repeat the same process; while others deposit upon the
walls of the capillaries their hyaline, which operates with fecun-

dating power upon cells lying in the parenchyma of the organs, and becomes assimilated according to the specific constitution of the same.   Sometimes, instead of chyle, as the fecundating substance to be assimilated, there reaches the hyaline of the blood-corpuscles quite another heterogeneous substance, for instance some sort of infectious matter, organic or animal poison, &c., whereby there as surely arise diseased processes of formation, which communicate themselves to the remaining portion of the blood or to the parenchyma of the organs.

The author refers to a full confirmation of his observations on the remarkable process of cell-formation in the germinal vesicle of the mammiferous ovum, by those of Mr. H. D. S. Goodsir on a cystic entozoon.   And as this lies at the other end of the series of organic existences, the operation of the process in question there, implies its operation in all intermediate ones.

He then notices an objection made to his observations, published in 1839 and 1840, when making known the fact that cleavage takes place in the *mammiferous* ovum also,—that such cleavage is effected by means of cells ; showing that inadequate research led to that objection, and concluding his remarks with the following words :—" After having examined 230 ova found in the Fallopian tube, with the sacrifice of 150 rabbits for embryological research, of which rabbits at least a score were devoted to anatomical inspection for the purpose of enabling me to determine the time at which the ovum leaves the ovary,—no man will wonder that I deem myself competent to judge whether the divisions of the germ are, or are not effected by means of cells. No man who does not examine mammiferous ova in large number immediately before their exit from the ovary, or otherwise through observations on animals or plants make himself acquainted with the germinal-spot-process of division, is able to comprehend the formative process in the mammiferous ovum in any of its earlier or later stages, or indeed *to understand the physiology of cells**."

A former drawing, fig. 13, shows the mode in which a spiral arises out of cells.   The following may serve to illustrate the way in which the *twin* or *double* spiral is produced.   Every microscopic observer must be familiar with segmented cytoblasts,

* [In the mammiferous ovum there is no substance that can be called a food-yelk.   The germ-cells therefore are not there obscured by a surrounding yelk-mass, the cleavage of which they *govern*, as seems to be the case in ova since figured by other observers.]

the annular arrangement of cell-germs in fig. 24, *b, c, d*.   Of
such rings of cell-germs, two are sometimes met with, connected
like two links of a chain, fig. 25.   Let the diagram fig. 26 *c*
represent a pile of such pairs of connected rings.   Now rings
such as those in fig. 24 are seen to pass into the state at *h* in
the same figure.   And this change occurring in each ring of the
pile of pairs of rings, fig. 26 *c*, with a uniting at the extremities
of rings lying one upon another, would produce the twin or
double spiral *d* in the same figure*.   Nature, it may be objected,
is a more skilful architect.   She does not first form rings in
order afterwards to divide them and unite their extremities in
another way.   All is from the first arranged in spiral order.
Without denying this, and fully admitting that there is from
the first a tendency to arrangement in spiral order, the author
still maintains that rings of cell-germs are constantly met with ;
and that since it is so ordered that spirals shall arise by the
union of separate cells, it is in perfect keeping with the form of
the cytoblast (fig. 24 *a*), that the germs of those cells when first
seen should be arranged in the form of rings.   (It must not be
forgotten that each of the rings entering into the formation of
the spiral has its centre of hyaline, whence the cell-germs of the
next generation of spirals. See fig. 13.)

That which in nutrition is ascribed exclusively to the fibrin of
the lymph (and which probably corresponds to the author's hya-
line), he believes to be derived from the blood-corpuscles them-
selves.   And it is his opinion, that in the coagulation of the
blood Nature gives us an example of the coagulation of the
blood-corpuscles; for, as he showed in 1842, *many fibres arise
through coagulation within those corpuscles*; whereby the latter
either pass entirely into fibres, as in the cytoblast blood-corpus-
cles of the Mammalia, or the coagulation takes place within
blood-cells, as in the other Vertebrata.

As already said, the reproduction of muscle seems to take
place by a process not differing essentially from that which
formed it, a process of division and subdivision of the germs of
cells.   And what are these germs of cells? They consist of
nothing less than that wondrous substance hyaline, the unceasing
maintenance of which the author believes to be the main purpose
in the formation and division of cells.   Each central row of cell-
germs within the windings of the spiral threads is really an axis-
cylinder of hyaline ; and when this divides, there arises a double

* [Or suppose a single pile of such bodies as that at *h* in fig. 24.   The
union of their extremities would produce a single spiral; and longitudinal
division of this single spiral would produce a double one.]

cylinder, and so on.   All these rows of cell-germs, arisen through division and subdivision of the nuclei of the primitive cells which arranged themselves in necklace-like order to form the first muscle tubes, as well as the germs of those primitive cells themselves, are descended through division from those substances in the ovum which again had arisen from the fecundated germinal spot or nucleus of the germinal vesicle.

In a brief recapitulation concerning hyaline, the author states his Researches in Embryology as well as his observations on the Corpuscles of the Blood (Phil. Trans. 1838, 1839, 1840, 1841), to have afforded him abundant opportunity for becoming acquainted with it*.   He found it in the so-called nucleolus of cells in general, as well as in the germinal spot of the germinal vesicle, to be the point of fecundation,—to be present in the head-like extremity of the spermatozoon,—to constitute as globules, immeasurably minute, the foundation of cytoblasts, these being the real germs of cells.   He showed that this hyaline forms as well the membrane as the contents of the cell,—that to it belong the functions of absorption, assimilation, and secretion,—that so long as the vegetative process is in full activity it never ceases to be in operation, but divides and subdivides to form new cells, or rather to reproduce itself.   For in the reproduction of cells, the maintenance, the division, and the increase of the hyaline appears to be the main purpose.   It may be asked, What is there, then, in the organic body which is not formed through hyaline? Truly nothing.   It is the essentially living substance in the body, the whole organism is the product of its formative force. All cell-germs are really, through repeated self-division, effected by a remarkable assimilative process, descendants of the hyaline of the germinal vesicle, this having been fecundated by a substance from the male ; whence the resemblance between the offspring and both its parents.   Finally, referring to his observations on the mode of origin and structure of nerve and other tissues, the author adds, that were it not that he would probably be blamed for excessive phantasy, he would not hesitate to declare the hyaline, as the foundation of the central nucleus of ganglion globules and of the axis-cylinders of nerves, to be the immediate organ of sensation of every kind.

[To be continued.]

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* See the Edinb. New Phil. Journ. Oct. 1843, a paper " On Fissiparous Generation;" and in the same Journal, Oct. 1847, another " On the Nucleus of the Animal and Vegetable Cell."

XII. *On the Occurrence of Berberine in the Columba Wood of Ceylon, the* Menispermum fenestratum *of Botanists.* By JAMES D. PERRINS, *Esq.*[*]

THE following investigation was made in the chemical laboratory of St. Bartholomew's Hospital under the immediate supervision of Dr. John Stenhouse. Dr. Stenhouse having had for some time past a quantity of the wood of the *Menispermum fenestratum* in his possession, suggested to me this investigation. I am anxious therefore to acknowledge my obligation to him, not only for the material, but also for several valuable suggestions in the course of the inquiry.

Hitherto the chief source of the alkaloid berberine has been the root of the barbery, *Berberis vulgaris.* Bödeker, however, about four years ago, ascertained its existence in the columba root of pharmacy, the *Cocculus palmatus,* where it occurs in small quantity associated with columbine. The following remark is made in the Chemical Gazette for 1849, vol. vii. p. 150:—
"The occurrence of berberine in *Berberis* and *Cocculus* is remarkable in a physiological point of view. Bartling places both these families, the Menispermeæ and Berberideæ, in the class of the Cocculinæ, which is in accordance with the fact of both containing the same principle." As berberine has now also been found in another of the Menispermæ, the accuracy of Bartling's view seems to be greatly confirmed.

The following was the process adopted for the extraction of berberine from the *Menispermum fenestratum.* A quantity of the wood, which had a bright yellow colour resembling that of quercitron, was rasped, and then treated with successive portions of boiling water till it had become nearly tasteless. The aqueous decoction acquired a deep yellow colour and an intensely bitter taste. It was next evaporated carefully to the consistence of an extract, then introduced into a flask and boiled with ten or twelve times its bulk of rectified spirits of wine, filtered while hot, and the residue boiled with a further quantity of spirits, which dissolved the berberine, and also a quantity of resinous matter by which it was accompanied. The alcoholic solution was then introduced into a retort, and the spirit carefully distilled off until the residue on agitation appeared to have nearly the consistence of oil of vitriol. It was then set aside in an open vessel, and in the course of twenty-four hours the liquid became filled with a mass of impure crystals.

After draining off the mother-liquor, these crystals were washed with a small quantity of cold spirit, redissolved in boiling alcohol, and set aside to crystallize. Their complete purification was

attempted by repeated crystallizations. It was found, however, that a small quantity of resinous matter adhered obstinately to the crystals, causing them to remain of a brownish-yellow colour. This brownish tint was ultimately entirely removed by solution in spirits of wine and digestion with a little purified animal charcoal, the pure berberine crystallizing from the solution in beautiful bright yellow needles. The crystals were found to contain nitrogen, and their behaviour with various reagents corresponded exactly with those of berberine.

As these crystals were very soluble in boiling water, a quantity of them was dissolved in that menstruum; and on the addition of the requisite amount of hydrochloric acid, a crystalline precipitate was immediately obtained in the form of long, slender, golden-coloured needles, of a fine silky lustre.

This salt was dried in a water-bath at 212° F., and subjected to analysis with the following results:—

6·25 grs., ignited with chromate of lead, gave 14·398 grs. of carbonic acid and 3·2 grs. of water.

The nitrogen was determined by Wills's method. 8·18 grs. of salt gave 4·94 grs. of the double chloride of platinum and ammonium.

The chlorine was determined as chloride of silver. 3·59 grs. gave 13·5 grs. of chloride of silver.

Hydrochlorate of Berberine.

			Calculated numbers.		Found numbers.
42 equivs.	Carbon	. .	3150	62·75	62·79
20 equivs.	Hydrogen	.	250	4·98	5·67
1 equiv.	Nitrogen	.	177	3·53	3·78
1 equiv.	Chlorine	. .	442	8·85	9·02
10 equivs.	Oxygen	. .	1000	19·90	
			5019	100·00	

These results correspond pretty closely with the formula of hydrochlorate of berberine, which, when dried at 212° F., contains 1 equiv. of water, and is consequently $C^{42} H^{18} NO^9$, HCl + HO.

The hydrogen in this determination is considerably too high, which however is easily accounted for, as the hydrochlorate of berberine, after being dried in the water-bath, is eminently hygroscopic, and consequently absorbs moisture rapidly while being mixed with the chromate of lead. This observation has already been made by Fleitmann, who, while analysing this salt, obtained an equally great excess of hydrogen.

A quantity of the double platinum salt was also prepared by mixing a solution of the hydrochlorate of berberine with one of chloride of platinum. The compound obtained corresponded

precisely in its appearance and properties with the salt prepared in the same way by Fleitmann.

2·80 grs. of salt gave 0·49 gr. of platinum = 17·5 per cent., the calculated quantity being 17·55 per cent.

A small quantity of the acid chromate of berberine was also prepared by adding a solution of bichromate of potash to one of hydrochlorate of berberine. The salt which precipitated likewise perfectly agreed in its properties with the acid chromate examined by Fleitmann.

The results of these analyses and reactions leave no doubt as to the identity of the alkaloid, and also serve to corroborate the correctness of Fleitmann's formula for berberine, which I briefly subjoin :—

Berberine crystallized at the ordinary temperature . . . $\quad\}\quad C^{42}H^{18}NO^9 + 12HO.$

Berberine dried at 212° F. . . . . $C^{42}H^{18}NO^9 + 2HO.$

The hydrochlorate dried at 212° F. $\quad C^{42}H^{18}NO^9 + HCl + HO.$

Double chloride of berberine and platinum . . . . . . $\quad\}\quad C^{42}H^{18}NO^9 + HCl + PtCl^2.$

The *Menispermum fenestratum* is, according to Ainslie, a large tree, which is very common in Ceylon, and an infusion of which has long been employed by the Cingalese as a valuable tonic bitter.

Gray, in his Supplement to the Pharmacopœia, informs us that this tree is known to the Cingalese by the names of Woniwol and Bangwellzetta.

Berberine may easily be obtained in very considerable quantity from Columba wood, the whole of which it pervades, and of which it is the colouring principle; and if, as I suspect, the resinous matter accompanying it consists chiefly of altered berberine, improved methods of extraction, such for instance as the employment of a vacuum pan apparatus, would in all probability still further augment the amount of product.

I am informed that berberine is employed as a remedial agent on the Continent, but its scarcity seems hitherto to have prevented its introduction into the medical practice of this country. As a good source for it has now been pointed out, it may be expected that berberine will take its place with the other alkaloids in our materia medica. To prevent misconception from the similarity of names, it may perhaps be as well to remark, that berberine and bebeerine are very different substances,—the latter being the active principle of the bark of the Bebeeree tree of Guiana, and as yet has not been obtained in a crystalline form.

St. Bartholomew's Hospital, July 20, 1852.

XIII. *On Artesian Wells near Silsoe in Bedfordshire.* By Edward
J. Chapman, *Professor of Mineralogy in University College,
London**.

THE interest attached at the present time to all questions
connected with deep wells as a source of water-supply,
induces me to offer the following brief notice of a locality rich in
salient examples of this nature. I am not aware that any account
of this locality has hitherto been published ; no mention, at least,
is made of it in Mr. Prestwich's able and elaborate work on the
water-bearing strata around London, although in the immediate
vicinity of districts to which he has particularly referred. The
site in question was first pointed out to me by Mr. Homersham,
the engineer to the London and Watford Spring-water Company,
with whom, in conjunction with Professor Clark of Aberdeen,
and Mr. Snoulton of Dover, I first visited it† ; but I regret that
other duties have not allowed me to bestow that full attention
upon the locality that the interest of the subject demands.

The village of Silsoe is situated on the inner edge of the outcrop
of the Lower Greensand formation—the higher division, or *étage
Urgonien* or *Aptien* of D'Orbigny—which at that point forms a
range of low hills running parallel, or roughly so, to the great chalk
escarpment of Bedfordshire and the adjoining counties. Between
the two hill-ranges lies an undulating valley, having a general
inclination towards the north-east, and possessing, in the parti-
cular locality here referred to, an average breadth of about three
miles. This valley consists of chalk-marl passing by almost in-
sensible gradations (through, it may be presumed, the upper
greensand equivalents) into gault ‡. The usual arenaceous cha-
racters of the upper greensand are, however, altogether undeve-
loped ; and the true gault clay, lithologically speaking, is only
met with in isolated patches of small extent. One of these occurs
near Lower Gravenhurst, producing bricks of the well-known
light colour, combined with great uniformity of texture and with
great sectility. Gault bricks and tiles may, in fact, be cut

* Communicated by the Author.
† The existence of deep wells in this neighbourhood was, I believe, made
known to Mr. Homersham by Mr. George Edwards of St. Albans, at one
time a resident in the locality. The water was presumed to come from the
chalk ; more especially as the surface streams of the valley are actually
derived from that source. Amongst other places at which the phænomenon
may be witnessed, a stream may be seen issuing from the chalk at "the
Bath," a picturesque spot about half a mile north of Barton Church. Slips
and faults on a large scale have evidently taken place along this portion of
the chalk escarpment.
‡ The transition of the gault into the calcareous clay may be traced more
particularly on the banks of the small stream-way near Ion Lodge, about a
quarter of a mile south-east of Wrest Park.

almost as readily as the "Bath stones" employed for household purposes.

In the accompanying section—between Silsoe (1) and the hills above Barton-in-the-Clay (2)—A represents the lower greensand, B the impermeable calcareo-argillaceous strata (gault, upper greensand, chalk marl), and C the middle chalk. Layers of drift coat these more or less; and on some of the intervening heights, as at Higham Gobion, &c., the top of the hill is capped by a thick mass

of chalk-like detritus mixed with rolled pebbles and a few fossils of the gault and other strata, underlying two or three feet of more modern alluvium or vegetable mould. The fossil specimens consist principally of belemnites (*B. minimus*, &c.), and of two or three species of Ostreæ. All of these are much rolled and water-worn, the Ostreæ presenting only single valves, and some of the larger belemnite guards being split longitudinally so as to show the phragmocone receptacle.

In other parts of the district, the drift-gravel contains pebbles of different kinds of granite, granular quartz, clay-slate, sandstone, iron-sandstone, flint, and iron pyrites converted into the hydrated sesquioxide of iron. The latter bodies are evidently derived from the chalk, similar nodules, or rust-stained cavities left by their entire decomposition, being seen in all the pits along the chalk range.

The lower greensand strata consist of soft and coarse sandstone beds, interstratified with bands of iron-sandstone and a few subordinate layers of clay. A section is exhibited in the quarry a little to the west of Silsoe Church. The beds—apart from their false stratification, of which peculiarity they offer an interesting example—are there seen to dip towards the south, thus constituting a large natural reservoir lying beneath the impermeable strata of the valley; and as the chalk-marl and gault series conjointly do not average more than 200 feet in actual depth, a surface supply of water is readily obtainable.

The beds of the valley are perfectly impermeable, and except where the patches of gault clay occur, they are of a chalk-like aspect. In this valley, within an area of about ten square miles, from one to two hundred borings have been executed; and in a great number of instances the bore-holes produce an overflowing stream. As a mean, it may be said that the water in a four-inch pipe rises about four or five feet above the ground; but this, of course, varies with the surface-configuration of the valley. In the higher parts, the water stands at a few feet below the surface,

but it remains constant at that level; whereas the ordinary wells sunk into the surface of the lower greensand, fluctuate in this respect, as might readily be imagined, with every change of season.

The bore-holes already executed vary in diameter from two to four inches, and do not require tubing beyond the first ten or twelve feet. Some of the smaller size, of an average depth of about 170 feet, have been put down at a cost not exceeding £7*.

The water derived from this subterranean reservoir is of a slightly chalybeate nature, depositing on exposure to the atmosphere a yellowish slime of hydrated sesquioxide of iron. Its temperature is 51° F. The ferruginous taste is at first strongly perceptible; but this, of course, becomes less apparent when the water has stood for some time, and I did not hear any complaints in respect to quality from persons in the daily habit of using the water for culinary and other purposes. On the contrary, it was generally considered to be very wholesome. Compared, indeed, to that taken from the brooks, in which organic matter is largely prevalent, the marked superiority of the deep-well water does not admit of the slightest doubt.

When first drawn, it is beautifully clear, but after the lapse of three or four hours it becomes clouded from separation of carbonate of iron; regaining, however, its transparency on the deposition of the precipitate. Mr. Dugald Campbell has kindly examined for me, by Dr. Clark's test, a specimen of this water taken from an overflowing well—shown in the annexed sketch—at Whitehall near Wrest Park, on the property of the Earl de Grey. This well is 186 feet deep, with a bore of four inches in diameter. The water gushes out with great force, day and night, in a continued stream, and at the rate of about 76 gallons a minute.
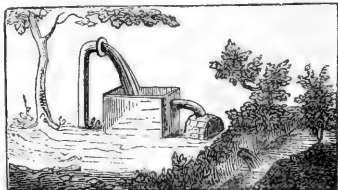
The following are the results obtained by Mr. Campbell :—

<div style="text-align:center">

Hardness . . . 9°·38

Alkalinity . . . 8°·50

</div>

It is difficult to arrive at any very accurate conclusions as to the quantity abstracted daily from this subterranean source ; but judging from the number and power of the overflowing wells, and from a fair estimate of the amount drawn from those which do not overflow, there must certainly be a daily consumption or

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* On the authority of William Arnold of Greenfield, by whom the greater part of the deep wells in this district were bored.

abstraction of at least three or four millions of gallons over the area indicated above. As these wells do not affect one another, however, in the slightest degree—and several of them have been flowing uninterruptedly for many years—we may fairly assume that the reservoir is capable of yielding a very much larger supply without detriment to existing interests.

There are perhaps few localities in which the subject of Artesian wells can be better studied than in this valley between Silsoe and Barton-in-the-Clay; the latter village lying under the bold escarpment of the chalk with its projecting spurs and rounded hollow coombes, and the former on the opposing range of the lower greensand. The relative heights, the mineral characters, and the dip of the strata are readily observable; and from various positions the eye can take in at a glance the physical and geological nature of the whole of the surrounding district—the marly impervious valley extending between the sandstone hills and the projecting chalk, and resting upon the under-dipping beds of the former strata.

In these examinations, besides other points of minor importance, five conditions have to be more particularly considered. First, the general levels of the country; secondly, the relative positions, inclination, and thickness of the strata; thirdly, their permeable or impermeable nature; fourthly, the outcrop area and surface configuration of the water-supplying beds; and fifthly, the chemical composition, &c. of the same, as likely to affect or not the quality of the water. Mineralogical characters, therefore, although useless, and even hurtful in their attempted interpretations, in questions of pure or abstract geology, become, in these local and practical investigations, of the highest value.

XIV. *On the Dynamical Theory of Heat, with numerical results deduced from* Mr. Joule's *equivalent of a Thermal Unit, and* M. Regnault's *observations on Steam. By* William Thomson, *M.A., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.*

Part II.—*On the Motive Power of Heat through Finite Ranges of Temperature.*

24. IT is required to determine the quantity of work which a perfect engine, supplied from a source at any temperature, S, and parting with its waste heat to a refrigerator at any lower temperature, T, will produce from a given quantity, H, of heat drawn from the source.

25. We may suppose the engine to consist of an infinite

number of perfect engines, each working within an infinitely small range of temperature, and arranged in a series of which the source of the first is the given source, the refrigerator of the last the given refrigerator, and the refrigerator of each intermediate engine is the source of that which follows it in the series. Each of these engines will, in any time, emit just as much less heat to its refrigerator than is supplied to it from its source, as is the equivalent of the mechanical work which it produces. Hence if t and $t + dt$ denote respectively the temperatures of the refrigerator and source of one of the intermediate engines, and if q denote the quantity of heat which this engine discharges into its refrigerator in any time, and $q + dq$ the quantity which it draws from its source in the same time, the quantity of work which it produces in that time will be $J dq$ according to Prop. I., and it will also be $q\mu dt$ according to the expression of Prop. II., investigated in § 21; and therefore we must have

$$J dq = q\mu dt.$$

Hence, supposing that the quantity of heat supplied from the first source, in the time considered is H, we find by integration

$$\log \frac{H}{q} = \frac{1}{J} \int_t^S \mu dt.$$

But the value of q, when $t = T$, is the final remainder discharged into the refrigerator at the temperature T; and therefore, if this be denoted by R, we have

$$\log \frac{H}{R} = \frac{1}{J} \int_T^S \mu dt \quad . \quad . \quad . \quad . \quad (5);$$

from which we deduce

$$R = H \epsilon^{-\frac{1}{J} \int_T^S \mu dt} \quad . \quad . \quad . \quad . \quad . \quad (6).$$

Now the whole amount of work produced will be the mechanical equivalent of the quantity of heat lost; and, therefore, if this be denoted by W, we have

$$W = J(H - R) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

and consequently, by (6),

$$W = JH \left\{ 1 - \epsilon^{-\frac{1}{J} \int_T^S \mu dt} \right\} \quad . \quad . \quad (8).$$

26. To compare this with the expression $H \int_T^S \mu dt$, for the duty indicated by Carnot's theory[*], we may expand the exponential in the preceding equation, by the usual series. We thus

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[*] "Account," &c., Equation 7, § 31.

find

$$W = \left(1 - \frac{\theta}{1.2} + \frac{\theta^2}{1.2.3} - \&c. \right) . H\int_T^S \mu dt \left.\vphantom{\int_T^S}\right\}$$

where

$$\theta = \frac{1}{J}\int_T^S \mu dt$$

$$(9).$$

This shows that the work really produced, which always falls short of the duty indicated by Carnot's theory, approaches more and more nearly to it as the range is diminished; and ultimately, when the range is infinitely small, is the same as if Carnot's theory required no modification, which agrees with the conclusion stated above in § 22.

27. Again, equation (8) shows that the real duty of a given quantity of heat supplied from the source increases with every increase of the range; but that instead of increasing indefinitely in proportion to $\int_T^S \mu dt$, as Carnot's theory makes it do, it never reaches the value JH, but approximates to this limit, as $\int_T^S \mu dt$ is increased without limit. Hence Carnot's remark * regarding the practical advantage that may be anticipated from the use of the air-engine, or from any method by which the range of temperatures may be increased, loses only a part of its importance, while a much more satisfactory view than his of the practical problem is afforded. Thus we see that, although the full equivalent of mechanical effect cannot be obtained even by means of a perfect engine, yet when the actual source of heat is at a high enough temperature above the surrounding objects, we may get more and more nearly the whole of the admitted heat converted into mechanical effect, by simply increasing the effective range of temperature in the engine.

28. The preceding investigation (§ 25) shows that the value of Carnot's function, μ, for all temperatures within the range of the engine, and the absolute value of Joule's equivalent, J, are enough of data to calculate the amount of mechanical effect of a perfect engine of any kind, whether a steam-engine, an air-engine, or even a thermo-electric engine; since, according to the axiom stated in § 12, and the demonstration of Prop. II., no inanimate material agency could produce more mechanical effect from a given quantity of heat, with a given available range of temperatures, than an engine satisfying the criterion stated in the enunciation of the proposition.

29. The mechanical equivalent of a thermal unit Fahrenheit, or the quantity of heat necessary to raise the temperature of a

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* " Account, &c." Appendix, Section IV.

pound of water from 32° to 33° Fahr., has been determined by Joule in foot-pounds at Manchester, and the value which he gives as his best determination is 772·69. Mr. Rankine takes, as the result of Joule's determination, 772, which he estimates must be within $\frac{1}{300}$ of its own amount, of the truth. If we take $772\frac{2}{9}$ as the number, we find, by multiplying it by $\frac{9}{5}$, 1390 as the equivalent of the thermal unit Centigrade, which is taken as the value of J in the numerical applications contained in the present paper.

30. With regard to the determination of the values of μ for different temperatures, it is to be remarked that equation (4) shows that this might be done by experiments upon any substance whatever of indestructible texture, and indicates exactly the experimental data required in each case. For instance, by first supposing the medium to be air; and again, by supposing it to consist partly of liquid water and partly of saturated vapour, we deduce, as is shown in Part III. of this paper, the two expressions (6), given in § 30 of my former paper ("Account of Carnot's Theory"), for the value of μ at any temperature. As yet no experiments have been made upon air which afford the required data for calculating the value of μ through any extensive range of temperature; but for temperatures between 50° and 60° Fahr., Joule's experiments[*] on the heat evolved by the expenditure of a given amount of work on the compression of air kept at a constant temperature, afford the most direct data for this object which have yet been obtained; since, if Q be the quantity of heat evolved by the compression of a fluid subject to "the gaseous laws" of expansion and compressibility, W the amount of mechanical work spent, and t the constant temperature of the fluid, we have by (11) of § 49 of my former paper,

$$\mu = \frac{W.E}{Q(1+Et)}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (10)$$

which is in reality a simple consequence of the other expression for μ in terms of data with reference to air. Remarks upon the determination of μ by such experiments, and by another class of experiments on air originated by Joule, are reserved for a separate communication, which I hope to be able to make to the Royal Society on another occasion.

31. The second of the expressions (6), in § 30 of my former paper, or the equivalent expression (32), given below in the present paper, shows that μ may be determined for any temperature from determinations for that temperature of—

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[*] "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," Phil. Mag. vol. xxvi. May 1845.

(1.) The rate of variation with the temperature, of the pressure of saturated steam.

(2.) The latent heat of a given weight of saturated steam.

(3.) The volume of a given weight of saturated steam.

(4.) The volume of a given weight of water.

The last mentioned of these elements may, on account of the manner in which it enters the formula, be taken as constant, without producing any appreciable effect on the probable accuracy of the result.

32. Regnault's observations have supplied the first of the data with very great accuracy for all temperatures between $-32°$ Cent. and 230°.

33. As regards the second of the data, it must be remarked that all experimenters, from Watt, who first made experiments on the subject, to Regnault, whose determinations are the most accurate and extensive that have yet been made, appear to have either explicitly or tacitly assumed the same principle as that of Carnot which is overturned by the dynamical theory of heat; inasmuch as they have defined the "total heat of steam" as the quantity of heat required, to convert a unit of weight of water at 0°, into steam in the particular state considered. Thus Regnault, setting out with this definition for "the total heat of saturated steam," gives experimental determinations of it for the entire range of temperatures from 0° to 230°; and he deduces the "latent heat of saturated steam" at any temperature, from the "total heat," so determined, by subtracting from it the quantity of heat necessary to raise the liquid to that temperature. Now, according to the dynamical theory, the quantity of heat expressed by the preceding definition depends on the manner (which may be infinitely varied) in which the specified change of state is effected; differing in different cases by the thermal equivalents of the differences of the external mechanical effect produced in the expansion. For instance, the final quantity of heat required to evaporate a quantity of water at 0°, and then, keeping it always in the state of saturated vapour*, bring it to the temperature 100°, cannot be so much as three-fourths of the quantity required, first, to raise the temperature of the liquid to

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* See below (Part III. § 58), where the "negative" specific heat of saturated steam is investigated. If the mean value of this quantity between 0° and 100° were $-1\cdot5$ (and it cannot differ much from this) there would be 150 units of heat emitted by a pound of saturated vapour in having its temperature raised (by compression) from 0° to 100°. The latent heat of the vapour at 0° being 606·5, the final quantity of heat required to convert a pound of water at 0° into saturated steam at 100°, in the first of the ways mentioned in the text, would consequently be 456·5, which is only about $\frac{4}{5}$ of the quantity 637 found as "the total heat" of the saturated vapour at 100°, by Regnault.

100°, and then evaporate it at that temperature; and yet either quantity is expressed by what is generally received as a *definition* of the "total heat" of the saturated vapour.   To find what it is that is really determined as "total heat" of saturated steam in Regnault's researches, it is only necessary to remark, that the measurement actually made is of the quantity of heat emitted by a certain weight of water in passing through a calorimetrical apparatus, which it enters as saturated steam, and leaves in the liquid state, the result being reduced to what would have been found if the final temperature of the water had been exactly 0°. For there being no external mechanical effect produced (other than that of sound, which it is to be presumed is quite inappreciable), the only external effect is the emission of heat.   This must, therefore, according to the fundamental proposition of the dynamical theory, be independent of the intermediate agencies. It follows that, however the steam may rush through the calorimeter, and at whatever reduced pressure it may actually be condensed*, the heat emitted externally must be exactly the same as if the condensation took place under the full pressure of the entering saturated steam; and we conclude that *the total heat,* as actually determined from his experiments by Regnault, is the quantity of heat that would be required, first to raise the liquid to the specified temperature, and then to evaporate it at that temperature; and that the principle on which he determines the latent heat is correct.   Hence, through the range of his experiments, that is from 0° to 230°, we may consider the second of

* If the steam have to rush through a long fine tube, or through a small aperture within the calorimetrical apparatus, its pressure will be diminished before it is condensed; and there will, therefore, in two parts of the calorimeter be saturated steam at different temperatures (as, for instance, would be the case if steam from a high pressure boiler were distilled into the open air); yet, on account of the heat developed by the fluid friction, which would be precisely the equivalent of the mechanical effect of the expansion wasted in the rushing, the heat measured by the calorimeter would be precisely the same as if the condensation took place at a pressure not appreciably lower than that of the entering steam.   The circumstances of such a case have been overlooked by Clausius (Poggendorff's *Annalen,* 1850, No. 4, p. 510), when he expresses with some doubt the opinion that the latent heat of saturated steam will be truly found from Regnault's "total heat," by deducting "the sensible heat"; and gives as a reason that, in the actual experiments, the condensation must have taken place "under the same pressure, or nearly under the same pressure," as the evaporation. The question is not, *Did the condensation take place at a lower pressure than that of the entering steam?* but, *Did* Regnault *make the steam work an engine in passing through the calorimeter, or was there so much noise of steam rushing through it as to convert an appreciable portion of the total heat into external mechanical effect?* And a negative answer to this is a sufficient reason for adopting *with certainty* the opinion that the principle of his determination of the latent heat is correct.

the data required for the calculation of μ as being supplied in a complete and satisfactory manner.

34. There remains only the third of the data, or the volume of a given weight of saturated steam, for which accurate experiments through an extensive range are wanting; and no experimental researches bearing on the subject having been made since the time when my former paper was written, I see no reason for supposing that the values of μ which I then gave are not the most probable that can be obtained in the present state of science; and, on the understanding stated in § 33 of that paper, that accurate experimental determinations of the densities of saturated steam at different temperatures may indicate considerable errors in the densities which have been assumed according to the "gaseous laws," and may consequently render considerable alterations in my results necessary, I shall still continue to use Table I. of that paper, which shows the values of μ for the temperatures $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$ . . . $230\frac{1}{2}$, or, the mean values of μ for each of the 230 successive Centigrade degrees of the air-thermometer above the freezing-point, as the basis of numerical applications of the theory. It may be added, that any experimental researches sufficiently trustworthy in point of accuracy, yet to be made, either on air or any other substance, which may lead to values of μ differing from those, must be admitted as proving a discrepancy between the true densities of saturated steam, and those which have been assumed[*].

35. Table II. of my former paper, which shows the values of $\int_0^t \mu dt$ for $t=1$, $t=2$, $t=3$, . . . $t=231$, renders the calculation of the mechanical effect derivable from a given quantity of heat by means of a perfect engine, with any given range included between the limits 0 and 231, extremely easy; since the quantity to be divided by J[†] in the index of the exponential in the

[*] I cannot see that any hypothesis, such as that adopted by Clausius fundamentally in his investigations on this subject, and leading, as he shows to determinations of the densities of saturated steam at different temperatures, which indicate enormous deviations from the gaseous laws of variation with temperature and pressure, is more probable, or is probably nearer the truth, than that the density of saturated steam does follow these laws as it is usually assumed to do. In the present state of science it would perhaps be wrong to say that either hypothesis is more probable than the other [or that the rigorous truth of either hypothesis is probable at all].

[†] It ought to be remarked, that as the unit of force implied in the determinations of μ is the weight of a pound of matter at Paris, and the unit of force in terms of which J is expressed is the weight of a pound at Manchester, these numbers ought in strictness to be modified so as to express the values in terms of a common unit of force; but as the force of gravity at Paris differs by less than $\frac{1}{8000}$ of its own value from the force of gravity at Manchester, this correction will be much less than the probable errors from other sources, and may therefore be neglected.

expression (8) will be found by subtracting the number in that table corresponding to the value of T, from that corresponding to the value of S. [Tables I. and II. of the former paper are reprinted here, for the sake of convenience in referring to them.]

*Tables extracted from "*Account of Carnot's Theory,*" Trans. R. S. Ed. vol. xvi. part 5.*

Explanation of Table I.

The mean values of μ for the first, for the eleventh, for the twenty-first, and so on, up to the 231st* degree of the air thermometer, have been calculated in the manner explained in the preceding paragraphs. These, and interpolated results, which must agree with what would have been obtained by direct calculation from Regnault's data to three significant places of figures (and even for the temperatures between $0°$ and $100°$, the experimental data do not justify us in relying on any of the results to a greater degree of accuracy), are exhibited in Table I.

To find the amount of mechanical effect due to a unit of heat, descending from a body at a temperature S *to a body at* T, *if these numbers be integers, we have merely to add the values of* μ *in Table* I. *corresponding to the successive numbers,*

$$\text{T}+1, \text{ T}+2, \ldots \text{ S}-2, \text{ S}-1.$$

Explanation of Table II.

The calculation of the mechanical effect in any case, which might always be effected in the manner described in § 37 (with the proper modification for fractions of degrees when necessary), is much simplified by the use of Table II., where the first number of Table I., the sum of the first and second, the sum of the first three, the sum of the first four, and so on, are successively exhibited. The sums thus tabulated are the values of the integrals

$$\int_0^1 \mu dt, \ \int_0^2 \mu dt, \ \int_0^3 \mu dt, \ldots \int_0^{231} \mu dt;$$

and if we denote $\int_0^t \mu dt$ by the letter M, Table II. may be regarded as a table of the values of M.

To find the amount of mechanical effect due to a unit of heat descending from a body at a temperature S *to a body at* T, *if these numbers be integers, we have merely to subtract the value of* M, *for the number* T + 1, *from the value for the number* S, *given in Table* II.

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* In strictness, the 230th is the last degree for which the experimental data are complete; but the data for the 231st may readily be assumed in a sufficiently satisfactory manner.

Table I*.—Mean Values of μ for the successive Degrees of the Air Thermometer from 0° to 230°.

°	μ.	°	μ.	°	μ.	°	μ.	°	μ.
1	4·960	48	4·366	94	3·889	140	3·549	186	3·309
2	4·946	49	4·355	95	3·880	141	3·543	187	3·304
3	4·932	50	4·343	96	3·871	142	3·537	188	3·300
4	4·918	51	4·331	97	3·863	143	3·531	189	3·295
5	4·905	52	4·319	98	3·854	144	3·525	190	3·291
6	4·892	53	4·308	99	3·845	145	3·519	191	3·287
7	4·878	54	4·296	100	3·837	146	3·513	192	3·282
8	4·865	55	4·285	101	3·829	147	3·507	193	3·278
9	4·852	56	4·273	102	3·820	148	3·501	194	3·274
10	4·839	57	4·262	103	3·812	149	3·495	195	3·269
11	4·826	58	4·250	104	3·804	150	3·490	196	3·265
12	4·812	59	4·239	105	3·796	151	3·484	197	3·261
13	4·799	60	4·227	106	3·788	152	3·479	198	3·257
14	4·786	61	4·216	107	3·780	153	3·473	199	3·253
15	4·773	62	4·205	108	3·772	154	3·468	200	3·249
16	4·760	63	4·194	109	3·764	155	3·462	201	3·245
17	4·747	64	4·183	110	3·757	156	3·457	202	3·241
18	4·735	65	4·172	111	3·749	157	3·451	203	3·237
19	4·722	66	4·161	112	3·741	158	3·446	204	3·233
20	4·709	67	4·150	113	3·734	159	3·440	205	3·229
21	4·697	68	4·140	114	3·726	160	3·435	206	3·225
22	4·684	69	4·129	115	3·719	161	3·430	207	3·221
23	4·672	70	4·119	116	3·712	162	8·424	208	3·217
24	4·659	71	4·109	117	3·704	163	3·419	209	3·213
25	4·646	72	4·098	118	3·697	164	3·414	210	3·210
26	4·634	73	4·088	119	3·689	165	3·409	211	3·206
27	4·621	74	4·078	120	3·682	166	3·404	212	3·202
28	4·609	75	4·067	121	3·675	167	3·399	213	3·198
29	4·596	76	4·057	122	3·668	168	3·394	214	3·195
30	4·584	77	4·047	123	3·661	169	3·389	215	3·191
31	4·572	78	4·037	124	3·654	170	3·384	216	3·188
32	4·559	79	4·028	125	3·647	171	3·380	217	3·184
33	4·547	80	4·018	126	3·640	172	3·375	218	3·180
34	4·535	81	4·009	127	3·633	173	3·370	219	3·177
35	4·522	82	3·999	128	3·627	174	3·365	220	3·173
36	4·510	83	3·990	129	3·620	175	3·361	221	3·169
37	4·498	84	3·980	130	3·614	176	3·356	222	3·165
38	4·486	85	3·971	131	3·607	177	3·351	223	3·162
39	4·474	86	3·961	132	3·601	178	3·346	224	3·158
40	4·462	87	3·952	133	3·594	179	3·342	225	3·155
41	4·450	88	3·943	134	3·586	180	3·337	226	3·151
42	4·438	89	3·934	135	3·579	181	3·332	227	3·148
43	4·426	90	3·925	136	3·573	182	3·328	228	3·144
44	4·414	91	3·916	137	3·567	183	3·323	229	3·141
45	4·402	92	3·907	138	3·561	184	3·318	230	3·137
46	4·390	93	3·898	139	3·555	185	3·314	231	3·134
47	4·378								

* The numbers here tabulated may also be regarded as *the active values* of μ for $t=\frac{1}{2}$, $t=1\frac{1}{2}$, $t=2\frac{1}{2}$, $t=3\frac{1}{2}$, &c.

Table II.—Mechanical Effect in Foot-Pounds due to a Thermic Unit Centigrade, passing from a body, at any Temperature less than 230° to a body at 0°.

Superior limit of temperature.	Mechanical effect.	Superior limit of temperature.	Mechanical effect.	Superior limit of temperature.	Mechanical effect.	Superior limit of temperature.	Mechanical effect.	Superior limit of temperature.	Mechanical effect.
°	ft.-lbs.	°	ft.-lbs.	°	ft.-lbs.	°	ft.-lbs.	°	ft.-lbs.
1	4·960	48	223·487	94	412·545	140	582·981	186	740·310
2	9·906	49	227·842	95	416·425	141	586·524	187	743·614
3	14·838	50	232·185	96	420·296	142	590·061	188	746·914
4	19·756	51	236·516	97	424·159	143	593·592	189	750·209
5	24·661	52	240·835	98	428·013	144	597·117	190	753·500
6	29·553	53	245·143	99	431·858	145	600·636	191	756·787
7	34·431	54	249·439	100	435·695	146	604·099	192	760·069
8	39·296	55	253·724	101	439·524	147	607·656	193	763·347
9	44·431	56	257·997	102	443·344	148	611·157	194	766·621
10	48·987	57	262·259	103	447·156	149	614·652	195	769·890
11	53·813	58	266·509	104	450·960	150	618·142	196	773·155
12	58·625	59	270·748	105	454·756	151	621·626	197	776·416
13	63·424	60	274·975	106	458·544	152	625·105	198	779·673
14	68·210	61	279·191	107	462·324	153	628·578	199	782·926
15	72·983	62	283·396	108	466·096	154	632·046	200	786·175
16	77·743	63	287·590	109	469·860	155	635·508	201	789·420
17	82·490	64	291·773	110	473·617	156	638·965	202	792·661
18	87·225	65	295·945	111	477·366	157	642·416	203	795·898
19	91·947	66	300·106	112	481·107	158	645·862	204	799·131
20	96·656	67	304·256	113	484·841	159	649·302	205	802·360
21	101·353	68	308·396	114	488·567	160	652·737	206	805·585
22	106·037	69	312·525	115	492·286	161	656·167	207	808·806
23	110·709	70	316·644	116	495·998	162	659·591	208	812·023
24	115·368	71	320·752	117	499·702	163	663·010	209	815·236
25	120·014	72	324·851	118	503·399	164	666·424	210	818·446
26	124·648	73	328·939	119	507·088	165	669·833	211	821·652
27	129·269	74	333·017	120	510·770	166	673·237	212	824·854
28	133·878	75	337·084	121	514·445	167	676·636	213	828·052
29	138·474	76	341·141	122	518·113	168	680·030	214	831·247
30	143·058	77	345·188	123	521·774	169	683·419	215	834·438
31	147·630	78	349·225	124	525·428	170	686·803	216	837·626
32	152·189	79	353·253	125	529·075	171	690·183	217	840·810
33	156·736	80	357·271	126	532·715	172	693·558	218	843·990
34	161·271	81	361·280	127	536·348	173	696·928	219	847·167
35	165·793	82	365·279	128	539·975	174	700·293	220	850·340
36	170·303	83	369·269	129	543·595	175	703·654	221	853·509
37	174·801	84	373·249	130	547·209	176	707·010	222	856·674
38	179·287	85	377·220	131	550·816	177	710·361	223	859·836
39	183·761	86	381·181	132	554·417	178	713·707	224	862·994
40	188·223	87	385·133	133	558·051	179	717·049	225	866·149
41	192·673	88	389·076	134	561·597	180	720·386	226	869·300
42	197·111	89	393·010	135	565·176	181	723·718	227	872·448
43	201·537	90	396·935	136	568·749	182	727·046	228	875·592
44	205·951	91	400·851	137	572·316	183	730·369	229	878·733
45	210·353	92	404·758	138	575·877	184	733·687	230	881·870
46	214·743	93	408·656	139	579·432	185	737·001	231	885·004
47	219·121								

36. The following tables show some numerical results which have been obtained in this way, with a few (contained in the lower part of the second table) calculated from values of $\int_0^t \mu\,dt$ estimated for temperatures above 230°, roughly, according to the rate of variation of that function within the experimental limits.

37. *Explanation of the Tables.*

Column I. in each table shows the assumed ranges.

Column II. shows ranges deduced by means of Table II. of the former paper, so that the value of $\int_T^S \mu dt$ for each may be the same as for the corresponding range shown in column I.

Column III. shows what would be the duty of a unit of heat if Carnot's theory required no modification (or the actual duty of a unit of heat with additions through the range, to compensate for the quantities converted into mechanical effect).

Column IV. shows the true duty of a unit of heat, and a comparison of the numbers in it with the corresponding numbers in column III. shows how much the true duty falls short of Carnot's theoretical duty in each case.

Column VI. is calculated by the formula

$$R = \epsilon^{-\frac{1}{1390}\int_T^S \mu dt},$$

where $\epsilon = 2\cdot71828$, and for $\int_T^S \mu dt$ the successive values shown in column III. are used.

Column IV. is calculated by the formula

$$W = 1390(1-R)$$

from the values of $1-R$ shown in column V.

38. *Table of the Motive Power of Heat.*

Range of temperatures.				III. Duty of a unit of heat through the whole range.	IV. Duty of a unit of heat supplied from the source.	V. Quantity of heat converted into mechanical effect.	VI. Quantity of heat wasted.
I.		II.					
S.	T.	S.	T.	$\int_T^S \mu dt.$	W.	$1-R.$	R.
				ft.-lbs.	ft.-lbs.		
1	0	31·08	30	4·960	4·948	·00356	·99644
10	0	40·86	30	48·987	48·1	·0346	·9654
20	0	51·7	30	96·656	93·4	·067	·933
30	0	62·6	30	143·06	136	·098	·902
40	0	73·6	30	188·22	176	·127	·873
50	0	84·5	30	232·18	214	·154	·846
60	0	95·4	30	274·97	249	·179	·821
70	0	106·3	30	316·64	283	·204	·796
80	0	117·2	30	357·27	315	·227	·773
90	0	128·0	30	396·93	345	·248	·752
100	0	138·8	30	435·69	374	·269	·731
110	0	149·1	30	473·62	401	·289	·711
120	0	160·3	30	510·77	427	·308	·692
130	0	171·0	30	547·21	452	·325	·675
140	0	181·7	30	582·98	476	·343	·657
150	0	192·3	30	618·14	499	·359	·641
160	0	203·0	30	652·74	521	·375	·625
170	0	213·6	30	686·80	542	·390	·610
180	0	224·2	30	720·39	562	·404	·596
190	0	190	0	753·50	582	·418	·582
200	0	200	0	786·17	600	·432	·568
210	0	210	0	818·45	619	·445	·555
220	0	220	0	850·34	636	·457	·542
230	0	230	0	881·87	653	·470	·530

39. *Supplementary Table of the Motive Powers of Heat.*

Range of temperatures.				III. Duty of a unit of heat through the whole range.	IV. Duty of a unit of heat supplied from the source.	V. Quantity of heat converted into mechanical effect.	VI. Quantity of heat wasted.
I.		II.					
S.	T.	S.	T.	$\int_T^S \mu dt.$	W.	$1 - R.$	R.
				ft.-lbs.	ft.-lbs.		
101·1	0	140	30	439·9	377	·271	·729
105·8	0	230	100	446·2	382	·275	·725
300	0	300	0	1099	757	·545	·455
400	0	400	0	1395	879	·632	·368
500	0	500	0	1690	979	·704	·296
600	0	600	0	1980	1059	·762	·238
∞	0	∞	0	∞	1390	1·000	·000

40. Taking the range 30° to 140° as an example suitable to the circumstances of some of the best steam-engines that have yet been made (see Appendix to Account of Carnot's Theory, Sec. v.), we find in column III. of the supplementary table, 377 ft.-lbs. as the corresponding duty of a unit of heat instead of 440, shown in column III., which is Carnot's theoretical duty. We conclude that the recorded performance of the Fowey-Consols engine in 1845, instead of being only $57\frac{1}{2}$ per cent. amounted really to 67 per cent., or $\frac{2}{3}$ of the duty of a perfect engine with the same range of temperature; and this duty being ·271 (rather more than $\frac{1}{4}$) of the whole equivalent of the heat used; we conclude further, that $\frac{1}{5\cdot49}$, or 18 per cent. of the whole heat supplied, was actually converted into mechanical effect by that steam-engine.

41. The numbers in the lower part of the supplementary table show the great advantage that may be anticipated from the perfecting of the air-engine, or any other kind of thermo-dynamic engine in which the range of the temperature can be increased much beyond the limits actually attainable in steam-engines. Thus an air-engine, with its hot part at 600°, and its cold part at 0° Cent., working with perfect œconomy, would convert 76 per cent. of the whole heat used into mechanical effect; or working with such œconomy as has been estimated for the Fowey-Consols engine, that is, producing 67 per cent. of the theoretical duty corresponding to its range of temperature, would convert 51 per cent. of all the heat used into mechanical effect.

42. It was suggested to me by Mr. Joule, in a letter dated December 9, 1848, that the true value of μ might be "inversely

as the temperatures from zero*;" and values for various temperatures calculated by means of the formula,

$$\mu = J \frac{E}{1 + Et}, \quad \ldots \quad \ldots \quad (11)$$

were given for comparison with those which I had calculated from data regarding steam. This formula is also adopted by Clausius, who uses it fundamentally in his mathematical investigations. If μ were correctly expressed by it, we should have

$$\int_T^S \mu \, dt = J \log \frac{1 + ES}{1 + ET};$$

and therefore equations (1) and (2) would become

$$W = J \frac{S - T}{\dfrac{1}{E} + S}, \quad \ldots \quad \ldots \quad (12)$$

$$R = \frac{\dfrac{1}{E} + T}{\dfrac{1}{E} + S}. \quad \ldots \quad \ldots \quad (13)$$

43. The reasons upon which Mr. Joule's opinion is founded, that the preceding equation (11) may be the correct expression for Carnot's function, although the values calculated by means of it differ considerably from those shown in Table I. of my former paper, form the subject of a communication which I hope to have an opportunity of laying before the Royal Society previously to the close of the present session.

[To be continued.]

* If we take $\mu = k \dfrac{E}{1 + Et}$ where k may be any constant, we find

$$W = J \left(\frac{S - T}{\dfrac{1}{E} + S} \right)^{\frac{k}{J}};$$

which is the formula I gave when this paper was communicated. I have since remarked, that Mr. Joule's hypothesis implies essentially that the coefficient k must be as it is taken in the text, the mechanical equivalent of a thermal unit. Mr. Rankine, in a letter dated March 27, 1851, informs me that he has deduced, from the principles laid down in his paper communicated last year to this Society, an approximate formula for the ratio of the maximum quantity of heat converted into mechanical effect to the whole quantity expended, in an expansive engine of any substance, which, on comparison, I find agrees exactly with the expression (12) given in the text as a consequence of the hypothesis suggested by Mr. Joule regarding the value of μ at any temperature.—[April 4, 1851.]

XV. *On the Chemical Constitution of Childrenite.* *By* Prof. RAMMELSBERG*.

AMONG the rarest crystallized minerals, and one that in a chemical point of view may be said to be hardly known at all, may be classed childrenite. This species, which was at first met with in very minute crystals, many years ago, in piercing a tunnel near Tavistock, has of late years been found in much finer specimens at the George and Charlotte Mine, some of the crystals indeed measuring full half an inch in length. Within the last twelvemonth, two specimens of a peculiarly dark colour have occurred at Wheal Crebor. Both these mines are situated near Tavistock.

On exposure to heat, childrenite gives off a considerable quantity of water. Before the blowpipe, it swells, and puts forth insulated branches, tinging the flame distinctly of a bluish-green colour, and forms a fissured, rounded mass, black in part, and in part brownish-red on the edges. With fluxes it gives the reaction of manganese and iron. In the form of a fine powder, the mineral is soluble by lengthened digestion in hydrochloric acid, leaving generally a slight residue consisting principally of quartz. The solution at length assumes a faint yellow colour; ammonia produces in it a voluminous dark blackish-green precipitate, which turns brown on exposure to the air, and which consists of phosphoric acid, alumina and the oxides of iron and manganese. The filtrate contains only phosphoric acid; there is no alkali in it. A freshly formed solution of the mineral showed a strong reaction of the protoxide of iron; of the peroxide the reaction was far less marked.

On exposure to a red heat in a covered platina crucible, the powdered childrenite loses its water. In one experiment, where the mineral was not altogether free from copper pyrites, this loss amounted to 16·35 per cent., a small quantity of sulphurous acid being given off. On employing the material in as pure a state as possible, the loss was 16·30 per cent. The powder, thus heated, is of a bluish-red, black internally; when heated with access of air, it is red throughout.

The loss of weight on exposure to heat corresponds to the amount of water in the mineral minus the oxygen, which the protoxide of iron (and of manganese) has taken up in its conversion into the peroxide.

As the crystals of childrenite are very firmly implanted on their gangue, which consists of carbonate of iron, quartz and

* From Poggendorff's *Annalen*, No. 3, 1852, with a few unimportant additions as to localities. Communicated by W. G. Lettsom, Esq.

copper pyrites, it is no easy matter to detach a sufficient quantity of them in a tolerable state of purity.

In the first analysis, the mineral in the state of powder having been exposed to heat, was fused with carbonate of soda and treated like a silicate. The precipitate thrown down by ammonia was exhausted several times with boiling potash, and then digested with hydrosulphate of ammonia to extract the whole of the phosphoric acid. After supersaturation with hydrochloric acid, the phosphate of alumina was precipitated from the potash solution by means of ammonia, the remainder of the phosphoric acid in the filtrate was next ascertained; the precipitate, after exposure to a red heat, was dissolved in acid, and the phosphoric acid was precipitated with chloride of magnesium.

1·229 grm., when thus treated, gave—

Silica	3·82	
Phosphoric acid	28·24	29·36
Alumina	18·06	18·77
Protoxide of iron	29·58	30·75
Protoxide of manganese . .	5·89	6·12
Oxide of copper	0·65	0·66
Loss by exposure to heat . .	16·35	17·00
	102·59	102·66

In a second analysis, 0·454 grm. was first exposed to a red heat, whereby the loss of weight amounted to 0·074.

2·804 grms. were next digested in hydrochloric acid, and left a residue of 0·113. The solution, after evaporation in the water-bath, was treated with ammonia and sulphuret of ammonium, the residue dissolved in acid, the solution oxidized, precipitated with ammonia, and the precipitate, after exposure to a red heat, was analysed by fusing it with silica and carbonate of soda.

After deducting the residue, the results obtained from this analysis, the materials for which were purer than those employed on the former one as given above, were—

			Oxygen.
Phosphoric acid	28·92		16·20
Alumina	14·44		6·74
Protoxide of iron	30·68	6·81 ⎫	
Protoxide of manganese . .	9·07	2·03 ⎬ 8·89	
Magnesia	0·14	0·14 ⎭	
Water	16·98		15·09
	100·23		

The quantities of oxygen are here in the ratio of 2·4 : 1 : 1·32 : 2·24. If instead we assume the ratio to be 2·5 : 1 : 1·32 : 2·5 = 15 : 6 : 8 : 15, and this we are the more justified in doing from the impossibility

of the entire amount of water being indicated by the loss from exposure to heat, childrenite may be looked upon as consisting of 8 atoms RO, 2 atoms alumina, 3 of phosphoric acid and 15 of water, which constitution is represented by the formula—

$$2(4RO, PO^5) + 2AlO^3, PO^5 + 15HO.$$

The first term in this formula is contained in triphyline, and triplite is asserted to have a similar composition.   The second term, with a third part of the amount of water, is met with in calaite.

It is to the kindness of Mr. Brooke, M. Krantz, and Mr. Lettsom that I am indebted for the materials on which I operated.

XVI. *Remarks on* Mr. Dresser's *Experiments on the Conducting Powers of Wires for Voltaic Electricity, and on* Mr. Joule's *Experiments with a powerful Electro-magnet.   By the* Rev. J. BASHFORTH, *Esq.*[*]

IN the September Number of the Philosophical Magazine, Mr. Dresser gave an account of some experiments with respect to the conducting powers of wires, which appeared to him to impugn the commonly received laws of their resistances to the galvanic current.   It is not rare to meet with objections to well-established laws arising from a misapprehension of their meaning, but objections of this kind are seldom founded on experiments so good as these appear to be.   Mr. Dresser has not explained how he compared the results of theory and experiment; but on Table I. he observes, " . . . . it is evident that the often quoted law of the conducting power of the wire being inversely as the length *does not obtain in short lengths*.   But there is an evident intimation of some other law, and probably different for different metals."   Again, " From this Table (II.), compared with Table I., it does not appear that with a thicker wire there is any nearer approach to the old law, but also that some other law obtains."   On Table III. it is remarked, that " Increase of intensity does not appear to approach near to the supposed law." And lastly, " This Table (IV.) does not coincide with the law of conduction of wires of different diameters being as the squares of the diameters."

It seems to me that all these erroneous conclusions spring from a wrong application of the laws quoted.   The galvanic current has other resistances to overcome besides that of the 1, 2,

3, &c. feet of wire which is introduced into the circuit for experimental purposes. It has to pass through the galvanometer wire, the nitric acid, the porous cell, the sulphuric acid, &c. We must therefore in our calculations suppose an addition of a length of wire to that which is used in the experiments, and which remains constant for one series of experiments. Thus suppose E the electromotive power of the battery, R the resistance of battery expressed by the length of wire (of the same kind as that used in the experiment), which would offer the same resistance as the fluids and solids of the battery actually do offer to the passage of the current. Let x denote the length of wire introduced into the circuit for the sake of experiment. Then we express the force of the current by $E \div (R + x)$, and not by $E \div x$, as has been very commonly and very erroneously done. From two observations, the values of E and R (which may approximately be supposed constant for one series of experiments) may be found; and then by giving to x the values 1, 2, 3 ... successively, we may obtain corresponding calculated deflections of the galvanometer which may be compared with the results of experiments. I have thus calculated all the four tables of experiments, and placed the results of theory and experiment side by side. The number of observations recorded in the last three tables is so small, that they are not of much real importance as tests.

As Mr. Dresser thought that he found the greatest deviation from the commonly received law when he experimented with short lengths of wires, I am led to the conclusion that he employed the formula $E \div x$ in his calculations. This would amount to a supposition, that the current had no other resistances to overcome beyond that arising from its passage through the wire introduced into the circuit, as had previously been done by Professor Barlow, I believe, and others. The absurdity of such a supposition will be seen by a simple illustration. Suppose that a person wished to find experimentally the law of resistance to a carriage carrying various numbers of passengers when propelled on a railway at a given velocity. Suppose that the tractive power required for 1, 2, 3 ... 20 passengers of equal weights was measured with great nicety, still very little confidence would be placed in any general law connecting the number of passengers and the tractive power when the heavy weight of the carriage itself was entirely neglected.

Table I.

Copper wire.
E=9253·32, R=22·2576.

Iron wire.
E=1848, R=4·6.

Length of wire in feet.	Observed deflection.	Difference.	Calculated deflection.	Error.	Length of wire.	Observed deflection.	Difference.	Calculated deflection.	Error.
1	398	18	397·9	+0·1	1	330	50	330·0	0·0
2	380	15	381·5	−1·5	2	280	40	280·0	0·0
3	365	13	366·4	−1·4	3	240	30	243·2	−3·2
4	352	12	352·4	−0·4	4	210	20	214·9	−4·9
5	340	10	339·5	+0·5	5	190	18	192·5	−2·5
6	330	10	327·5	+2·5	6	172	14	174·3	−2·3
7	320	24	316·3	+3·7	7	158	23	159·3	−1·3
9	296	10	296·0	0·0	9	135	7	135·8	−0·8
10	286	17	286·9	−0·9	10	128	16	126·6	+1·4
12	269	15	270·1	−1·1	12	112	12	111·3	+0·7
14	254	14	255·2	−1·2	14	100	8	99·4	+0·6
16	240	10	241·9	−1·9	16	92	7	89·8	+2·2
18	230	10	229·9	+0·1	18	85	7	81·8	+3·2
20	220	...	219·0	+1·0	20	78	5	75·1	+2·9
					22	73	5	69·5	+3·5
					24	68	4	64·6	+3·4
					26	64	4	60·4	+3·6
					28	60	...	56·7	+3·3

Table II.

Copper.
E=11660, R=40·4.

Iron.
E=2764·8, R=9·8.

Length of wire.	Observed deflection.	Difference.	Calculated deflection.	Error.	Length of wire.	Observed deflection.	Difference.	Calculated deflection.	Error.
1	282	7	281·6	+0·4	1	256	21	256·0	0·0
2	275	7	275·0	0·0	2	235	18	234·3	+0·7
3	268	6	268·7	−0·7	3	217	17	216·0	+1·0
4	262	6	262·6	−0·6	4	200	13	200·3	−0·3
5	256	5	256·8	−0·8	5	187	12	186·8	+0·2
6	251*	5	251·3	−0·3	6	175	11	175·0	0·0
7	246*	5	246·0	0·0	7	164	7	164·6	−0·6
8	241*	...	240·9	+0·1	8	157	...	155·3	+1·7

Table III.

Copper wire.
E=18800, R=46.

Iron wire.
E=3382·83, R=8·529.

Length of wire.	Observed deflection.	Difference.	Calculated deflection.	Error.	Length of wire.	Observed deflection.	Difference.	Calculated deflection.	Error.
1	400	9	400·0	0·0	1	355	35	355·0	0·0
2	391	9	391·7	−0·7	2	320	26	322·0	−2·0
3	382	6	383·7	−1·7	3	294	24	293·4	+0·6
4	376	6	376·0	0·0	4	270	18	270·0	0·0
5	370	...	368·6	+1·4	5	252	...	250·0	+2·0

* These three numbers were apparently erroneously printed in the published account of the experiments: the differences there given have been used.

Table IV.

Copper wire.   Iron wire.
E = 125·1845, R = 0·58582.   E = 23·370, R = ·104.

Diameter of wire.	Observed deflection.	Calculated deflection.	Error.	Diameter of wire.	Observed deflection.	Calculated deflection.	Error.
370	190	190	0·0	300	129	129	0·0
480	195	199·05	−3·95	510	165	164·2	+0·8
700	206½	206·5	0·0	640	182	182·0	0·0
740	207	207·2	−0·2	720	188	189·5	−1·5

The agreement between calculation and experiment above shown is far nearer than could have been expected, considering the many sources of error. The experiments must have been very carefully conducted.

Lenz in 1832 showed by experiment with convolutions of wire of diameters 0·73 inch, 6·57 inches, and 28 inches, that the electromotive power which *magnetism* produces in them remains the same (*Mém. Acad. St. Pét.*; translated in the Scientific Memoirs, vol. i. p. 607). Again, in 1838, Lenz and Jacobi published accounts of experiments, which showed that the magnetism excited by the galvanic current in a long bar of iron, by comparatively short spirals surrounding it, was, for a given strength of current, independent of the diameter of the spiral. It appears that Mr. Joule was guided by this principle in the construction of his powerful electro-magnets. Still it must not be concluded that a wide convolution is *on the whole* as advantageous practically as a smaller one. The resistance of a wire varies as its length ; consequently the wider the spirals, the more the strength of the galvanic current is diminished for a given number of convolutions. Thus with Mr. Dresser's battery, and the iron wire of Exp. I., the effect of one circuit, 1 foot long, would be 330 ; of one 2 feet long, 280; of one 3 feet long, 240, &c.; and of one 28 feet long, 60.

It appears that Mr. Joule took no precautions for ascertaining the exact strengths of the galvanic current with which he experimented. The arrangements of the sixteen cells which he adopted were certainly calculated to produce currents as 1, 2 and 4 ; but the disturbing causes are so numerous, that in every case an actual measurement was absolutely necessary. Again, the number of observations in each experiment is far too small ; for within the limits of errors which Mr. Joule appears to allow, it would be possible to confirm a great variety of laws. In Exp. I. the arc of vibration is not given, but in Exp. II. it is said to have been as large as a *quadrant* of a circle. Such a circumstance as this deserved some explanation. The resistance of the air to the bar of bismuth, making seventeen vibrations per minute through

an angle of 90°, must have been considerable.   Mr. Joule seems to have forgotten, that when a body oscillates under the action of any force, that force varies as the square of the numbers of vibrations only under *very peculiar circumstances*.   In this experiment a bar of bismuth, $1\frac{5}{8}$ inch long, vibrated between the two temporary poles of the electro-magnet $1\frac{1}{4}$ inch apart. The force which caused the bismuth to vibrate is supposed to have been dependent on induction; and consequently during every vibration of the bismuth bar through 90°, the force which Mr. Joule was desirous of estimating must have gone through very wide variations.   It is certain that the angle of vibration, instead of being 90°, ought to have been the *least possible* for the purpose of obtaining any satisfactory comparisons of forces resulting from a variation in the strengths of the galvanic current.

The number of vibrations per minute observed in Exp. I. with assumed currents 1, 2 and 4, were 48, 63 and 96.   In Exp. VI. the number of vibrations with a strength of current 2 was found to be 63, as before.   On this Mr. Joule remarks, that these three numbers are evidently as the square roots of 1, 2 and 4.   Now 48, 67·87 (= 63 + 4·87) and 96 are really as the square roots of 1, 2 and 4.   If we assume that the forces vary as the square of the observed number of vibrations, we get 1, 1·72 and 4, instead of 1, 2 and 4.   On plotting Mr. Joule's results of Exp. I., I obtained three points very nearly in a straight line; and I am thus led to a law which the experiment does very nearly satisfy, namely,

$$30\tfrac{1}{2} \div 1 \times 16 = 48 - \tfrac{1}{2}, \quad 31\tfrac{1}{2} + 2 \times 16 = 63 + \tfrac{1}{2},$$

and
$$31\tfrac{1}{2} + 4 \times 16 = 96 - \tfrac{1}{2}.$$

I merely mention this as a fact, without any intention to propose it as a general law.

The assumed currents of 1, 2 and 4, in Exp. II., gives $4\frac{1}{4}$, $9\frac{1}{2}$, and 17 vibrations through *a quadrant* per minute; and hence if we supposed the forces of the electro-magnet to vary as the number of vibrations, we should have 1, 2·23 and 4 for the strengths of the galvanic current, instead of 1, 2 and 4.   Thus if we suppose that the commonly received laws hold good, and from Mr. Joule's numbers of observed vibrations, endeavour to deduce the strengths of the galvanic currents employed, in Exp. I. we get 1, 1·72 and 4, in Exp. II. 1, 2·23 and 4, instead of 1, 2, and 4 in both cases.   It is remarkable, that in both experiments consistent results are obtained in the first and last observations; but that the middle observations in the same two experiments deviate very considerably, in *opposite* directions, from the desired results.

On Exp. III. Mr. Joule remarks, that " In this instance we

notice a slight falling away from the theoretical attraction," owing no doubt to the gradual approach to the limit of magnetizability in the small bar of iron. And concerning Exp. IV., it is said that "Here again we have evidences of an approach towards the *limit of magnetizability,* for the attractions with a current of 4 are only *ten times* instead of *sixteen times* as great as those observed with a current of 1." Experiments of this kind are most difficult to perform in a satisfactory manner; and certainly these can add little to our knowledge on this point after the appearance of Dub's paper on the subject in Poggendorff's *Annalen**, which furnishes details of very numerous and most carefully conducted experiments with various cylindrical electromagnets and keepers. Much depends on the size and form of the keeper; for Dub found that, by merely changing the form of a keeper (the mass remaining constant), the lifting power of the same electro-magnet varied between limits of 1 and 10 at least. Again, much must depend on the soft iron of the electro-magnet itself; and therefore no great confidence can be placed in the result of Exp. V., which assigns the maximum attractive power *per square inch of surface* of an electro-magnet. Mr. Joule does not appear to have tried a sufficient variety of forms, both of magnet and keeper, to warrant him in fixing this limit.

From Experiments I. and II. we see no indication of an approach to the limits of magnetizability; but with the same currents and magnet we find that this is the reason given for the wide departure of experimental from theoretical attractions. Now from the first two experiments it is manifest that there could only be an approach to the limit of magnetizability *in the keeper* in Exp. IV.; consequently a more massive keeper should have been tried afterwards; and this was the more necessary, as some recent experimenters have denied the existence of such a limit.

The rule for comparing the lifting powers of two *similar* electro-magnets does not appear to be by any means satisfactory. As we are not told in what this *similarity* is supposed to consist, it is impossible to test the theoretical correctness of the rule. The two electro-magnets compared by Mr. Joule were provided with 60 and 100 lbs. of coils of copper wire, and with 10 and 16 cells respectively. Therefore by the rule, the attractive powers ought to be as $60 \times 10 : 100 \times 16 = 6 : 16 = 1 : 2\cdot67$. The attractive powers were found to be in the following ratios:—

At a distance of $\frac{1}{4}$ inch as $480 : 976 = 1 : 2\cdot03$

$\qquad$ ... $\qquad \frac{1}{2}$ ... $\quad 168 : 320 = 1 : 1\cdot90$

$\qquad$ ... $\qquad 1$ ... $\quad 77 : 140 = 1 : 1\cdot82$

St. John's College, Cambridge.

* Phil. Mag., March 1851.

XVII. *On the Electrical Condition of the Atmosphere.*
By Reuben Phillips, *Esq.**

182. HAVING in former papers proved that the friction of air against water produces electricity, and having shown that the origin of atmospheric electricity can be found in the friction of wind on drops of rain, I inferred that the upper regions of the air were left by the descent of rain in a negative condition. I look upon the following as a direct proof of the existence of this negative state of the atmosphere, which leads to some other conclusions.

183. The positive electricity which the earth receives from lightning and rain must rapidly find its way back to the atmosphere, for the earth is electrically neutral under ordinary circumstances, as is known by comparing its state with the absolute electric zero†. Again, the atmosphere when explored by rods or kites gives a positive charge when the air is clear; therefore the air itself, so far as the modes of exploration extend, is positively electrified. But since the earth is neutral, the upper regions must be negative; or we have a quantity of positive electricity existing in the atmosphere, and its equivalent of negative electricity nowhere—which latter supposition the whole body of electrical science forbids.

184. The atmosphere, then, may be regarded as consisting of two spherical orbs, the lower one positively, and the upper one negatively electrified; and these two orbs induce towards each other, leaving the earth neutral. Now the positive electricity will, both from conduction and convection, continually travel upwards. This seems to explain the fact, that the positive electricity is stronger at some distance than at the surface of the earth.

185. Since the positive and negative orbs of the atmosphere induce towards each other, they must mutually attract. This attraction is counterbalanced by the elasticity of the atmosphere; consequently it is difficult not to admit that the atmosphere is condensed by the electric force between the mutually attractive volumes. There are not, so far as I am aware, any data whereby to determine what the absolute intensity of the electrical state of the atmosphere is, and it is therefore impossible to assign any value to the amount of condensation thus effected. Aurora may, perhaps, sometimes be occasioned by electricity forcing its way along and between these oppositely electrified orbs, as well as by its coming to the earth. The general tendency of the air to rise in the warmer, and, as a concomitant result, its downward tendency in higher latitudes, is possibly not without its effect in

* Communicated by the Author.
† Faraday, Phil. Mag. vol. xxix. p. 257.

producing aurora, by bringing the superior portions of the air nearer to the earth.

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186. *On atmospheric electrical maxima and minima.*—" It has been ascertained by the observations of De Saussure, Schubler, Arago and others, that the positive electricity of the atmosphere is subject to diurnal variations of intensity, there being two maxima and two minima during the twenty-four hours. The first minimum takes place a little before the rising of the sun ; as it rises, the intensity, at first gradually and then rapidly, increases, and arrives at its first maximum a few hours after. This excess diminishes at first rapidly and afterwards slowly, and arrives at its minimum some hours before sunset ; it reascends when the sun approaches the horizon, and attains its second maximum a few hours after, then diminishes till sunrise, and proceeds in the order already indicated. The intensity of the free electricity of the atmosphere has also been found to undergo annual changes, increasing from the month of July to the month of November inclusive, so that the greatest intensity occurs in winter, and the least in summer*."

187. When the sun has but a small elevation above the horizon, its rays enter the atmosphere so as to traverse large distances of air, and the rays are thereby more freely absorbed by the air at the place of observation than when the sun stands higher ; and at sunrising, the clouds which may have formed during night will still further intercept the sun's heat ; and it is obvious, that the air at little distances from the ground will be less heated than the superior strata, which receive the more undiminished rays. Now the effect of this will be to produce an expansion of the atmosphere, and the expansion of the higher strata will be greater than that of the lower strata ; consequently that part of the negative atmospheric orb, over-head at the place of observation, becomes lifted up, and more separated from the inferior positive orb than it was before sunrise ; and the appearance presented to us is that of an increased positive tension of the atmosphere, just as when the plates of an ordinary condenser are separated. And this, I think, gives rise to the first maximum after sunrise. With regard to the minimum which next occurs, it must be observed, that as the sun ascends, the proportion of its rays absorbed by the ground increases, both absolutely and in comparison with the quantity absorbed by the atmosphere during their passage through it. Therefore the lower strata of the atmosphere now become more especially the seat of the expansion produced by the sun's heat ; the result of which is, that the over-head portion of the positive orb is uplifted, and its influ-

ence on projecting rods is consequently diminished, a minimum being at last obtained.

188. When the sun is about to set, the temperature of the air near the ground falls, and consequently the positive orb is lowered, while the sun's rays still heat the upper regions of the air as at sunrise, and the negative orb is also upborne by the expansion previously communicated during the day; and when the sun is set, the temperature of the air near the ground rapidly falls. Here are then the conditions for another maximum. Lastly, during night, the temperature of the superior regions of the atmosphere falls, which causes the portion of the negative orb to descend nearer to the positive, and thus produces a diminished electrical effect; as when the plates of a condenser are closed.

189. The annual variation is evidently connected with the quantity of rain. Thus the quantity of electricity is greater in winter than in summer, because more rain falls in the autumnal than in the summer months. As far as I can see, there are only two conditions under which rain or hail can ever be produced without begetting static electricity; one is, when the friction to which the drops of water are exposed is so small that the quantity of electricity produced is virtually nothing; the other is, when the insulation of the different parts of this natural electric machine is so imperfect that the dynamic effect only is obtained. And most probably neither of these two conditions is ever very strictly fulfilled.

On the Colours of a Jet of Steam.

190. Professor Forbes some years ago observed, that a jet of steam absorbed the more refrangible portion of white light[*]. It happened during some experiments, that a blue jet of steam caught my attention, and further experiments soon assured me that it was easy to obtain a jet of almost any colour.

191. A blowpipe jet was screwed on a T-piece, and the *opposite* opening of the T-piece was supplied with a stopcock, while the third opening of the T-piece communicated, by means of a tube, with the cock of the boiler. The blowpipe jet had an orifice about $\frac{3}{100}$ths of an inch diameter, and its axis was elevated about 28° above the horizon. The stopcock on the T-piece was furnished with a little contrivance, for preventing the steam that it discharged from interfering with the appearance of the steam discharged by the blowpipe jet; the use of this stopcock was to blow off the water which condensed in the steam passages. A pressure was maintained in the boiler of about 40 lbs. on the inch.

192. On fully opening the cock of the boiler, a jet of steam was obtained which appeared blue in nearly every position in

[*] *Philosophical Magazine*, S. 3. vol. xiv. p. 121.

which it could be viewed. Looking end on from below, the steam-jet caused that part of the heavens obscured by it to appear feebly orange-coloured—the day was bright, but the sky at this quarter was overcast. On looking through the jet of steam from below upwards, but in a direction inclined about 11° to the axis of the jet—in which position a portion only of the steam-cloud could be viewed by the direct light of the clouds, the remaining portion being sheltered by the side of the window—one part of the jet appeared orange-red, namely that part which transmitted the direct light of the clouds, while the other portion was blue. The blueness of the jet increased with the above-mentioned angle until the angle was perhaps 30°, after which the blueness some-what diminished, but was far from being extinguished at 90°.

193. By partly closing the cock of the boiler, and so dischar-ging steam from the jet of, perhaps, not a higher pressure than 10 lbs. on the inch, I could obtain a jet of steam, which, looking end on from below, was blue. It was rather difficult to obtain this blue jet, and when obtained, it kept alternating with violet. On now viewing this blue jet under an angle, as before (192.), of about 20°, it appeared reddish-orange in colour; this colour was not visible at almost any angle, like the reflected blue (192.).

194. Looking end on, and adjusting the pressure, I have occasionally seen for a moment at a time a bright green jet; also, and commonly, a blue purple. In the reflected tints I am not sure that I have seen anything more than orange-red, violet and blue. The transmitted colour appeared in my experiments more intense than the reflected tints. This, perhaps, has its explana-tion in the fact, that when looking end on, the eye receives light which has shone through a columnar arrangement, whose length is much greater than its diameter,—while the reflected lights could only be seen by looking on the convex surface of the co-lumnar stream of particles.

195. Prof. Forbes, after discovering the red colour of a jet of steam by transmitted light, connected the red colour of the clouds with this fact; and the truth of this connexion is beyond dispute. So far, however, as I have been able to go, the colours of the steam-jet are manifestly only instances of ordinary inter-ference, greatly resembling that produced by thin transparent plates; the transmitted ray being always complementary to the reflected. Thus in (192.) the transmitted light is red, as in Prof. Forbes's experiments, but the reflected light is blue. It is therefore to be inferred, that all the colours of the clouds ori-ginate in interference, caused by minute drops of water, the size of which determines their colour; while the blue jet (192.) is, I think, strictly analogous to the blue sky.

7 Prospect Place, Ball's Pond Road,
       June 28, 1852.

XVIII. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity.* By M. DONOVAN, *Esq., M.R.I.A.*

SECTION VII.

ONE of the properties supposed in the early period of the history of galvanism to constitute a difference between the voltaic and electric agents, was the great power of the former to cause chemical decomposition, and the total inefficiency, as was then believed, of the latter. Dr. Wollaston first showed that ordinary electricity could, by peculiar methods, be made to effect a few decompositions, although with difficulty. Faraday, a few years since, found means to effect decompositions with greater facility, thus removing, as he conceived, one of the chief objections of those who still denied the identity of the two agents.

It is not necessary to describe Faraday's experiments in detail. His general method was to place paper soaked in a solution of the substance to be acted on between the extremities of two platinum wires, one connected with the positive conductor of an electric machine, through which the current of electricity entered the solution, and the other with a discharging train consisting of the gas-pipes and water-pipes of the street. On passing the electrical current through the arrangement, he states that the elements arranged themselves round the wire which transmitted, and the wire which received the electricity, in the same manner as they would have done if the same substances had been submitted to decomposition by the positive and negative poles of the voltaic battery. This he conceived to be further confirmation of the opinion, that the two kinds of decomposition are produced by the same agent.

It appears to me that these experiments go just as far to prove difference as identity of the two agents. When a compound body is to be decomposed by a voltaic series, the polar wires are placed in contact with the body; the results are, that the elements of the compound separate and arrange themselves into two classes, and each class collects round its proper pole. So constant a result is this division of bodies into two great orders, that Berzelius has founded on it a classification of elements into electro-positive and electro-negative.

Such is the effect of the two polar wires; but their conjoint effect is indispensable; remove either, and the other is powerless,—the decomposition and all other symptoms of energy cease. The negative wire attracts the more numerous denomination of bodies round it; but both are equally important. Mr. Gassiot

has strikingly proved the necessity of the direct application of both poles, if proof were wanted, by perfectly insulating 320 cells of his great water-battery, connecting one pole with the ground, and the other with solution of iodide of potassium. In this state no decomposition resulted; but when both polar wires were placed in connexion with the iodide, decomposition took place, and iodine was freely evolved*. Faraday has given another instance: if two insulated voltaic troughs be placed in the same right line, the two adjoining ends being connected by a wire over which is suspended a magnetic needle, there will be no deflection; but as soon as the two distant ends are connected, deflection takes place†.

In the decompositions effected by Faraday by means of common electricity, it is remarkable that he made use of but one pole, and that the positive. The wire connected with the positive conductor of the electrical machine was the only real and legitimate pole concerned. The wire which carried off the electricity to the gas and water-pipes was not in the negative state; and if of sufficient thickness, was not in any electric state; for it transmitted to the general reservoir as fast as it received the electricity, and manifested no electrical properties. It is very true, that if an insulated conductor be approached within a short distance of one that is also insulated and electrified, the former will become electrical by induction, one end being positive, the other negative; but if the conductor thus electrified by induction be made to communicate with the ground, its electrical state is destroyed. In the case of Faraday's wire which carried off electricity to the discharging train, it cannot be supposed to be in the negative state caused by induction; for induction can only be manifested when an insulating medium is interposed between the two conductors concerned‡. In the present instance, the interposed medium was a saline solution, an excellent conductor of the electricity which an excited electrical machine is capable of transmitting in a current. Beside all this, induction should not enter into the comparison of frictional with voltaic electricity, as no such thing is known or acknowledged in voltaic electricity unless as an hypothesis. Here, then, was decomposition with one pole only; and hence there was no analogy with voltaic decomposition, for this always requires the cooperation of the two poles.

Should any suspicion relative to the influence of induction be entertained, Faraday's own words prove that he did not conceive such an influence to be in operation. A bit of turmeric paper,

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* Phil. Mag., Oct. 1844, p. 290.   † Researches, par. 282.

‡ " Induction can only take place through or across insulators."—Faraday's Researches, par. 1678.

about half an inch square, was moistened with solution of sulphate of soda, and placed on the edge of a glass plate within about two inches of a point connected with the discharging train. The end of a decomposing wire proceeding from the prime conductor rested on the turmeric paper. The machine being put in action, positive electricity passed through the decomposing wire, in at one end of the turmeric paper, and out again at the other end towards the distant point of the discharging train[*]. Here he expressly admits that nothing but positive electricity acted at the two extremities of the very small bit of turmeric paper; no negative electricity or negative pole could be concerned; yet after forty or fifty turns of the machine, the red stain on the end of the turmeric paper which discharged positive electricity towards the point indicated the presence of alkali derived from the decomposition of the sulphate of soda. It is therefore true that one pole, and one kind of electricity only, here produced a decomposition, which therefore is of a different nature from one effected by voltaic electricity.

This was still more evident in an experiment where a large strip of turmeric paper, wet with solution of sulphate of soda, was hung from the prime conductor. On working the machine, alkali was developed at that part where the positive electricity was discharged from the paper[†].

In these cases, the alkali appeared at what, in point of fact, was the positive pole as it was carrying positive electricity, contrarily to the voltaic law, according to which it should have appeared only at the negative pole if there had been one. The experiments, therefore, instead of supporting, seem to discountenance the alleged identity.

In other experiments no pole whatever was employed, positive electricity being received from the air at one end of the paper, and the same electricity discharged into the air from the other end: surely there is here no analogy with voltaic decomposition.

Professor Faraday nowhere refers to the employment of a positive and negative pole in any one experiment; on the contrary, he says that the discharging train with its point "*represents*" the negative pole[‡].

The objections here made may be simplified and reduced to one by the following variation of the experiment on decomposition. I placed a bit of turmeric paper well soaked in solution of sulphate of soda, and the redundant liquid drained off, on the positive conductor of a powerful electric machine. The end of

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[*] "The machine was then worked, the positive electricity passing into the turmeric paper at the point *p*, and out at the extremity *n*."—Researches, par. 462.

[†] Ibid. par. 464.   [‡] Ibid. par. 454.

a wire was gently pressed upon the paper, the other end was held in the hand, and the cylinder was made to revolve. In a few moments alkali made its appearance under the wire, and rendered the paper brown. Is not the operation of negative electricity here excluded? is not the alkali detached by positive electricity? and is not the alkali found in a situation the opposite of that which is indicated by the law of voltaic decomposition?

Faraday's peculiar views relative to the current lead him to believe that positive and negative electricity are always coexistent and inseparable in it: hence the law of voltaic decomposition is conceived differently by him; and the evolution of alkali at the positive pole, and of acid at the negative, is not an irreconcileable result, although such a distribution is contrary to that acknowledged by Davy, Berzelius, and all the original experimenters on this subject. If Faraday's view be correct, and if the electricity at both poles of a voltaic series be the same, viz. consisting of both positive and negative electricity, it is difficult to assign a cause for the separate appearance of the two classes of bodies at their respective poles; it is difficult also to understand why, in Faraday's experiments, alkali or acid should have been developed at all: if they were separated from their combination, they should have appeared together in the same spot of the paper, and no change of colour should have ensued. More of this hereafter.

With regard to voltaic electricity, he describes an elegantly executed experiment* in which it was proved that decomposition can be effected by the voltaic series when one of the poles is in contact with the saline solution to be decomposed, and the other with water lying as a stratum over the saline solution; thus intending to demonstrate that the separation and collection of the elements round the poles are not attributable to any attractive power of the polar wires or other conductors. But this experiment does not obviate the objections which I have made against the alleged proofs of the identity of frictional and voltaic electricity, inasmuch as two effective poles were really in operation, although one of them acted through a quantity of water, while the other acted directly; the water was virtually the pole; or if not, the virtue of the polar wire was exerted through it.

The final inference to be drawn from all the experiments is, that to produce voltaic decompositions, the two different kinds or states of electricity must be in operation mediately or immediately. In all the experiments ever made there is not, within my knowledge, one which dispenses with this condition; while it appears that, in the case of common electricity, one kind or state is sufficient. The experiments in question cannot therefore be admitted as proofs of identity, although they may of difference.

* Researches, par. 494.

It is very probable that Professor Faraday was induced to adopt the discharging train, as a substitute for the negative pole, by his peculiar views of the nature of voltaic excitement.   In his theory, it is assumed that the current of electricity is generated by the chemical action of the oxygen of the exciting fluid on the zinc, a state of polar tension being the result, which is immediately relieved by the conducting power of the copper plate; and through this plate the electricity passes off.   Here it is evident that the duty assigned to the copper plate is that of a mere conductor; and this is precisely the function attributed to the copper wire attached to the discharging train in the experiments with common electricity just described.   Such, however, appears inconsistent with some well-known facts.   On this subject Professor Poggendorff observes, that were the office of the copper plate that of a mere conductor, it should follow that, as copper is the best conductor of electricity, a circle of copper and zinc should form the most powerful battery, and platinum and zinc should be much inferior, whereas the direct contrary is the fact; for if the conducting power of copper be taken according to Davy's estimate at 100, that of platinum will be but $16\frac{1}{2}$.

To the argument of Poggendorff I may add, that if the copper plate in the series acted as a mere conductor, a stout wire of copper should answer as well; which is so far from being the case, that in Wollaston's battery the power is greatly increased by the adoption of a plate of copper double the size of what is commonly used.   The copper is folded round the zinc; but that this is not the cause of the increased energy of the battery, is shown by the experiments of Mr. Binks.   This gentleman also proves, that an increase of the copper beyond Wollaston's double plate is accompanied by a still greater display of power[*].

There are some facts connected with the decomposition of chemical compounds by common electricity which seem still further to dissociate that agent from the voltaic.   Several eminent authorities have expressed their conviction, that chemical and electric attraction are different exhibitions of the same power.   This opinion, first promulgated by Volta, was adopted and amplified by Sir H. Davy, and has been accredited by Faraday, Berzelius, Ampère and others, with more or less modification. Before I proceed to my argument, it may be remarked that this doctrine leads to some speculations which do not appear to correspond with notions tacitly admitted, or at least not questioned by these philosophers.   An electric machine will emit sparks twelve or twenty inches long.   Are these to be considered sparks of affinity? can a flash of lightning be otherwise named a flash of affinity? and what is the stream of light into which a spark passed through the Torricellian vacuum resolves

[*] Phil. Mag. vol. xi. New Series, p. 75.

itself? Is it affinity independent of matter, separated from and no longer a property of matter, an existence *per se*? These are questions not devised for the purpose of procuring startling admissions, but difficulties which naturally present themselves and demand an explanation. It answers no good purpose and means nothing, to reply that electricity and affinity are different exhibitions of the same power.

Well, granting that electricity is all these things, one of its properties, insisted on as contradistinguishing the frictional from the voltaic modification, is its high intensity. If, then, frictional electricity be affinity at a high degree of intensity, how comes it to pass that its decomposing powers are so trivial compared with its intensity? How is it explicable, that, in the electrotype process, a single pair of plates will reduce many ounces of copper from its solution in a few days, the intensity of any electricity in operation being so feeble as to be inappreciable; while the utmost power of the largest electrical machine would absolutely never effect the same object? Why is the most feeble intensity of voltaic electricity so effective, when the intensity of frictional electricity, perhaps a hundred times more energetic, is comparatively powerless, and required the ingenuity of a Wollaston or a Faraday to make it act at all? In this argument the consideration of quantity may be omitted; we do not recognise quantity of affinity; all we are acquainted with is strength and intensity of affinity. Were it granted that the affinity which holds together the elements of a grain of water is electricity amounting to a flash of lightning, as supposed by Faraday, then indeed the attraction might be understood to be so intense as to resist decomposition by frictional electricity; for in that case a vast quantity would be concentrated within a small bulk. But were this true, how could frictional electricity, under any circumstances, decompose water?

In this view of the subject, it is most strange that, although a wire of zinc and a wire of platinum joined can decompose acidulated water voltaically, the utmost power of a large electrical machine, acting through the same wires connected with both conductors, will prove ineffectual, unless some devices be made use of which altogether change the character of the proceeding; and even then the effects are trifling. The enormous power of a hydro-electric machine is necessary to procure the decomposition, with voltaic arrangement of the elements. Here, then, Faraday's law fails, namely, that "the chemical power of a current is in direct proportion of the absolute quantity of electricity which passes*." It is true that he makes the case of water when acted on by common electricity an exception

* Researches, par. 821.

to his law*. The essence of a law of nature is its universality; if there be an exception, the alleged law is not a constituent ordination in the organization of the universe. Should we not confine the application of this law, if it be really a law, to the operations of the voltaic agent, whatever it may be, and withdraw the decomposition of water by frictional electricity altogether from its comprehension, claiming its refractory comportment as a proof of difference rather than of identity?

The decomposition of water, accompanied by voltaic arrangement of the elements which the hydro-electric machine is known to effect, offers no objection to the foregoing reasoning; the fact only proves, what is nowhere denied in this essay, that in all electricity there is an admixture of the constituent element which imparts to voltaic phænomena their characteristic chemical powers. Nay, the necessity of such an enormous quantity and intensity of frictional electricity for the decomposition of water is in itself a presumption that the real agent is not the electricity proper, but some other constituent, of which the former is but the vehicle. If the electric fluid, amongst its other elementary constituents, contain even the most minute portion of that chemical agent which in voltaic phænomena produces the chief part, a continued torrent of sparks ought to produce just the small evidences of chemical action which we observe. Perhaps the small degree of chemical action which large quantities of frictional electricity exercise, affords us the best measure of the real decomposing agent present. Instances will be given in a subsequent part of this essay, where the ratio of the chemical constituent appears to predominate over what I have called the electricity proper, as much as, in frictional electricity, the latter does over the former.

There is a point of view in which the relation of electricity to affinity must be considered with the object of questioning whether decompositions effected either by ordinary or voltaic electricity are explicable by, and compatible with the doctrine of *electrochemical equivalents*, viz. that "the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being electricity which determines the equivalent number, because it determines the combining force. Or if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them†." Hence we learn, that, according to this view, the atoms of matter are all endued with equal quantities of electricity,

and that these quantities constitute their affinity for each other*.

Although Professor Faraday has not expressed himself fully on this subject, I understand him to mean, that of combining atoms some are naturally in the positive state (*i. e.* naturally associated with a positive electricity or affinity), and some in the negative state; and indeed he expresses himself elsewhere† to this precise effect. An atom of each of the different kinds of matter will contain an equal quantity or degree of a different state or kind of electricity; that is, will have an electric attraction equal with that of every other atom in the same electric state, for every atom that is naturally in a differently electric state. But if all the atoms of matter are endued with equal forces of electrical attraction, then they are all endued with equal forces of affinity; and if all atoms, that is all matter, be endued with equal force of affinity, then there can be no superior or inferior forces of affinity, and therefore chemical decomposition due to this cause can never take place. Mixtures of incompatible substances may therefore be made without exchange of principles; all the affinities will be quiescent, because not any one of the elements concerned will possess stronger powers of affinity than another.

How, then, can the decomposing powers of electricity, whether frictional or voltaic, be reconciled to the doctrine of electrochemical affinity and electro-chemical equivalents? If the atoms of all bodies be associated with equal quantities of electricity or affinity, and if during decomposition "the quantity (of electricity) which passes is the equivalent of, and therefore equal to that of the particles separated‡," it is not easy to understand why the equal quantity of electricity that is supposed to act should produce separation of the elements previously united by a force exactly similar.

On this subject Baron Berzelius observes, that "according to this hypothesis, the same electric current which is adequate to the separation of an atom of silver from an atom of oxygen would also separate an atom of potassium from an atom of

* This idea, in a somewhat different form, had occurred to Sir H. Davy in 1826, but he did not follow it up. That philosopher says, "in assuming the idea of two æthereal, subtile, elastic fluids, attractive of the particles of each other, and repulsive as to their own particles, *capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities, and rendering them equivalent masses*, it would be natural to refer the action of the poles to the repulsions of the substances combined with the excess of one fluid, and the attractions of those united to the excess of the other fluid, and a history of the phænomena not unsatisfactory to the reason might in this way be made out."—*Philosophical Transactions*, 1826.

† Researches, par. 961.   ‡ Ibid. par. 855.

oxygen, although the former combination is one of the weakest, and the latter one of the strongest with which we are acquainted*."

If Faraday's electro-chemical equivalent numbers represent neutralizing quantities of electricity, as atomic numbers represent saturating ratios of combinable bodies, it should happen that combination of different kinds of matter, in atomic ratios, should take place without development of free electricity, the two states having been in all cases exactly sufficient to neutralize each other. But the contrary condition of chemical combination is notorious; for there are few instances of chemical action which are not accompanied by the evolution of electricity, and such evolution is easily recognizable, provided the bodies concerned are conductors. Instances in abundance are furnished by the experiments of Lavoisier, Laplace, Becquerel, Pouillet and others.

<div style="text-align:center">[To be continued.]</div>

XIX. *A Demonstration of the Theorem that every Homogeneous Quadratic Polynomial is reducible by real orthogonal substitutions to the form of a sum of Positive and Negative Squares. By* J. J. Sylvester, *Barrister-at-Law*†.

IT is well known that the reduction of any quadratic polynomial

$$(1, 1)x^2 + 2(1, 2)xy + (2, 2)y^2 + \&c. \ldots + (n, n).t^2$$

to the form $a_1 . \zeta^2 + a_2 . \eta^2 + \ldots + a_n . \theta^2$, where $\zeta, \eta, \ldots \theta$ are linear functions of $x, y, \ldots t$, such that $x^2 + y^2 + \ldots + t^2$ remains identical with $\zeta^2 + \eta^2 + \ldots \theta^2$ (which identity is the characteristic test of orthogonal transformation), depends upon the solution of the equation

$$\begin{vmatrix} (1, 1) + \lambda & (1, 2) & \ldots & (1, n) \\ (2, 2) & (2, 2) + \lambda & \ldots & (2, n) \\ \cdot & \cdot & \cdot & \cdot \\ (n, 1) & (n, 2) & \ldots & (n, n) + \lambda \end{vmatrix} = 0.$$

The roots of this equation give $a_1, a_2, \ldots a_n$; and if they are real, it is easily shown that the connexions between $x, y, \ldots t$; $\zeta, \eta, \ldots \theta$, are also real. M. Cauchy has somewhere given a proof of the theorem‡, that the roots of λ in the above equation must necessarily always be real; but the annexed demonstration

is, I believe, new; and being very simple, and reposing upon a theorem of interest in itself, and capable no doubt of many other applications, will, I think be interesting to the mathematical readers of the Magazine.

Let

$$f(\lambda) = \begin{vmatrix} (1, 1)+\lambda & (1, 2) & \dots & (1, n) \\ (2, 1) & (2, 2)+\lambda & \dots & (2, n) \\ (3, 1) & (3, 2) & (3, 3)+\lambda \dots (3, n) \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ (n, 1) & (n, 2) & \dots & (n, n)+\lambda \end{vmatrix}$$

it is easily proved that $f(\lambda) \times f(-\lambda)$

$$= \begin{vmatrix} [1, 1]-\lambda^2 & [1, 2] & \dots & [1, n] \\ [2, 1] & [2, 2]-\lambda^2 \dots [2, n] \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ [n, 1] & [n, 2] & \dots & [n, n]-\lambda^2, \end{vmatrix}$$

where

$$[\iota, \epsilon] = (\iota, 1) \times (1, \epsilon) + (\iota, 2) \times (2, \epsilon) + \dots + (\iota, n) \times (n, \epsilon).$$

If, now, for all values of r and s $(r, s) = (s, r)$, *i. e.* if $f(0)$ becomes the complete determinant to a symmetrical matrix, then every term $[r, s]$ in the derived matrix becomes a sum of squares, and is essentially positive, and $(-1)^2 . f(\lambda) \times f(-\lambda)$ assumes the form

$$(\lambda^2)^n - F(\lambda^2)^{n-1} + G(\lambda^2)^{n-2} + \dots \pm L,$$

where F, G, ... L will evidently be all positive; for it may be shown that F will be the sum of the squares of the separate terms, *i. e.* of the last minor determinants of the given matrix, G the sum of the squares of the last but one minors, and so on, L being the square of the complete determinant. For instance, if

$$f(\lambda) = \begin{vmatrix} a+\lambda & \gamma & \beta \\ \gamma & b+\lambda & \alpha \\ \beta & \alpha & c+\lambda \end{vmatrix}$$

$$-f(\lambda) \times f(-\lambda) = \lambda^6 - F\lambda^4 + G\lambda^2 - H,$$

where

$$F = a^2 + b^2 + c^2 + 2\alpha^2 + 2\beta^2 + 2\gamma^2$$
$$G = (ab - \gamma^2)^2 + (bc - \alpha^2)^2 + (\beta^2 - ac)^2$$
$$\qquad + 2(a\alpha - \beta\gamma)^2 + 2(b\beta - \gamma\alpha)^2 + 2(c\gamma - \alpha\beta)^2$$
$$H = \begin{vmatrix} a & \gamma & \beta \\ \gamma & b & \alpha \\ \beta & \alpha & c \end{vmatrix}^2$$

Hence it follows immediately that $f(\lambda)=0$ cannot have imaginary roots ; for, if possible, let $\lambda=p+q\sqrt{-1}$, and write

$$a+p=a' \quad b+p=b' \quad c+p=c' \quad \lambda+p=\lambda',$$

$f(\lambda)$ becomes

$$\begin{vmatrix} a'+\lambda' & \gamma & \beta \\ \gamma & b'+\lambda' & \alpha \\ \beta & \alpha & c'+\lambda' \end{vmatrix}$$

or say $\phi(\lambda')$, and the equation $\phi(\lambda') \times \phi(-\lambda')=0$ will be of the form $\lambda'^6 - F'.\lambda'^4 + G'\lambda'^2 - H'=0$, where F', G', H' are all essentially positive. Hence, by Descartes' rule, no value of λ'^2 can be negative, *i. e.* $(\lambda-p)^2$ cannot be of the form $-q^2$; that is to say, it is impossible for any of the roots of $f(\lambda)=0$ to be impossible, or, as was to be demonstrated, all the roots are real.

I may take this occasion to remark, that by whatever linear substitutions, orthogonal or otherwise, a given polynomial be reduced to the form $\Sigma A_1 \zeta^2$, the number of positive and negative coefficients is invariable : this is easily proved. If now we proceed to reduce the form (expressed under the umbral notation) $(a_1 x_1 + a_2 x_2 + \ldots + a_n . x_n)^2$ to the form

$$A_1 \zeta_1^2 + A_2 \zeta_2^2 + \ldots + A_{n-1}. \zeta_{n-1}^2 + A_n . x_n^2,$$

by first driving out the mixed terms in which x_1 enters, then those in which x_2 enters, and so forth until eventually only x_n of the original variables is left, it may readily be shown that

$$A_1 = \frac{a_1}{a_1} \quad A_2 = \frac{a_1 a_2}{a_1 a_2} \div \frac{a_1}{a_1} \quad A_3 = \frac{a_1 a_2 a_3}{a_1 a_2 a_3} \div \frac{a_1 a_2}{a_1 a_2}$$

$$\ldots A_n = \frac{a_1 \; a_2 \ldots a_n}{a_1 \; a_2 \ldots a_n} \div \frac{a_1 \; a_2 \ldots a_{n-1}}{a_1 \; a_2 \ldots a_{n-1}}.$$

It follows, therefore, that in whatever order we arrange the umbræ $a_1 \; a_2 \ldots a_n$, the number of variations and of continuations of sign in the series

$$1, \; \frac{a_1}{a_1} \; \frac{a_1 a_2}{a_1 a_2} \ldots \frac{a_1 \; a_2 \ldots a_n}{a_1 \; a_2 \ldots a_n}$$

will be invariable, and in fact will be the same as the number of positive and negative roots in the generating function in λ above treated of, *i. e.* since all the roots are real, will be the same as the number of variations and continuations in the series formed by the coefficients of the several powers of λ, *i. e.*

$$1, \; \Sigma\frac{a_1}{a_1}, \; \Sigma\frac{a_1 a_2}{a_1 a_2} \ldots \frac{a_1 a_2 . a_n}{a_1 a_2 . a_n}.$$

The first part of this theorem admits of an easy direct demonstration ; for by my theory of compound determinants, given in

this Magazine, we know that

$$\overline{a_1 \; a_2 \ldots a_{r-1} \, a_r} \quad \overline{a_1 \; a_2 \ldots a_{r-1} \cdot a_{r+1}}$$
$$a_1 \; a_2 \ldots a_{r-1} \cdot a_r \quad a_1 \; a_2 \ldots a_{r-1} \cdot a_{r+1}$$
$$= \begin{pmatrix} a_1 & a_2 \ldots a_{r-1} \\ a_1 & a_2 \ldots a_{r-1} \end{pmatrix} \times \begin{pmatrix} a_1 & a_2 \ldots a_{r-1} \, a_r \cdot a_{r+1} \\ a_1 & a_2 \ldots a_{r-1} \, a_r \cdot a_{r+1} \end{pmatrix}.$$

The first member of this equation is equivalent to

$$\begin{pmatrix} a_1 & a_2 \ldots a_{r-1} \, a_r \\ a_1 & a_2 \ldots a_{r-1} \, a_r \end{pmatrix} \times \begin{pmatrix} a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \\ a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \end{pmatrix}$$
$$- \begin{pmatrix} a_1 & a_2 \ldots a_{r-1} \, a_r \\ a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \end{pmatrix}^2.$$

Hence it follows, that if the two factors on the right-hand side of the equation have the same sign,

$$\frac{a_1 \; a_2 \ldots a_{r-1} \, a_r}{a_1 \; a_2 \ldots a_{r-1} \, a_r} \quad \text{and} \quad \frac{a_1 \; a_2 \ldots a_{r-1} \, a_{r+1}}{a_1 \; a_2 \ldots a_{r-1} \, a_{r+1}}$$

have also the same sign *nter se,* and consequently the two triads

$$\begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \\ a_1 & a_2 \ldots a_{r-1} \end{bmatrix} \begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \, a_r \\ a_1 & a_2 \ldots a_{r-1} \, a_r \end{bmatrix} \begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \, a_r \, a_{r+1} \\ a_1 & a_2 \ldots a_{r-1} \, a_r \, a_{r+1} \end{bmatrix}$$

and

$$\begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \\ a_1 & a_2 \ldots a_{r-1} \end{bmatrix} \begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \\ a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \end{bmatrix} \begin{bmatrix} a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \, a_r \\ a_1 & a_2 \ldots a_{r-1} \, a_{r+1} \, a_r \end{bmatrix}$$

will in all cases present the same number of changes and continuations, which proves that the contiguous umbræ, a_r, a_{r+1}, may be interchanged without affecting the number of variations and continuations in the entire series; but, as is well known, any one order of elements is always convertible into any other order by means of successive interchanges of contiguous elements, which demonstrates that, in whatever order the elements $a_1 \, a_2 \ldots a_n$ be arranged, the number of continuations and variations in

$$1, \; \frac{a_1}{a_1}, \; \frac{a_1 a_2}{a_1 a_2} \ldots \frac{a_1 a_2 \ldots a_n}{a_1 a_2 \ldots a_n}$$

is invariable. But that the same thing is true (as we know it to be), for the relation between any one of these unsymmetrical series and the symmetrical series (resulting from the method of orthogonal transformation)

$$1, \; \Sigma \frac{a_1}{a_1}, \; \Sigma \frac{a_1 a_2}{a_1 a_2}, \ldots \Sigma \frac{a_1 a_2 \ldots a_n}{a_1 a_2 \ldots a_n},$$

is by no means so easily demonstrable in the general case by a direct method, and the attention of algebraists is invited to supply such direct method of demonstration. My knowledge of the

fact of this equivalence is, as I have stated, deduced from that remarkable but simple law to which I have adverted, which affirms the invariability of the number of the positive and negative signs between all linearly equivalent functions of the form $\Sigma \pm c_r \cdot x^r$ (subject, of course, to the condition that the equivalence is expressible by means of equations into which only real quantities enter) ; a law to which my view of the physical meaning of quantity of matter inclines me, upon the ground of analogy, to give the name of the Law of Inertia for Quadratic Forms, as expressing the fact of the existence of an invariable number inseparably attached to such forms.

26 Lincoln's-Inn-Fields,
    July 12, 1852.

XX. *Early Egyptian Chemistry.* *By* J. DENHAM SMITH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

PERMIT me to occupy a small space in your Journal with a few observations on Mr. Herapath's paper with the above title, published in your Supplement Number for July.

Mr. Herapath's *fact* of a solution of silver having been used some three thousand years since as "marking-ink," is in every way interesting, being excellent additional evidence of the familiarity of the ancient Egyptians with a somewhat advanced stage of the chemical arts; but dissenting as I do from *all* the deductions Mr. Herapath has arrived at from this fact, and thinking it probable that they may take deep root and become widely spread as received opinions, if unremarked upon, I have ventured to allege a few reasons in refutation of the inferences of your correspondent.

The first conclusion necessarily involved in the views of Mr. Herapath is, that the ancient Egyptians must have been acquainted with nitric acid; the second is, that they were familiar with the use of sulphuric and hydrochloric acids; the third, that the Great Lawgiver travelled with what must be considered a well-appointed laboratory, or, which is still less probable, was able to construct an impromptu one (both materials and apparatus) in the Desert; and the fourth, that the golden calf was dissolved in aqua regia; all of which conclusions are founded and built up on the single fact of the existence on mummy linen of marks which must have been produced by a solution of silver.

To the two first, (the third I need not notice) I would object that there is no nation of antiquity, with whose every-day existence, their manners, customs and arts, we are so well acquainted as with those of the ancient Egyptians; and that, whilst we

have abundant evidence of their familiar and skilful practice of many metallurgic arts, there is no representation nor evidence whatever,—I speak under correction,—of their acquaintance with the art of distillation; and I hold that, in this instance, the absence of such evidence amounts to a *primâ facie* proof that they were ignorant of it. How these acids were to be obtained without distillation, Mr. Herapath does not inform us. I have here taken the most favourable supposition, that the presumed Egyptian sulphuric acid was obtained by distillation, like the Nordhausen acid, rather than by any complicated processes similar to those employed in the present day.

Again, if it can be shown that the Egyptians of those times were acquainted with substances capable of producing a solution of silver, it is surely advisable to pause before adopting a theory involving the employment of various materials and several complicated processes, of which, excepting silver and common salt, there is no evidence whatever they knew of, and take Horace's counsel—

> "Never presume to make a god appear,
>   But for a business worthy of a god."

With silver, and consequently with its ores, with common salt, and with lime, it will at once be admitted that this nation was familiar; and although it is probably incapable of proof that ammonia was known to them, yet if we consider that sal-ammoniac was for ages derived exclusively from Egypt, being procured from the soot of camel's-dung used as fuel, a necessity, and consequently a practice, which must have existed in the Mosaic epoch as well as now, since no other fuel is procurable in the Desert, together with the unchangeableness of eastern habits, and the fact that this salt was known to the writer of the earliest authentic chemical treatise extant, it is scarcely assuming too much to believe that sal-ammoniac was employed in the arts in ancient Egypt; and with these four substances, as every chemist knows, a solution of silver may readily be procured without the intervention of nitric, or indeed of any acid whatever; which solution is decomposed by exposure to air and light, particularly if in contact with an organic body, with the production of dark purple-black stains. It must not be supposed, because an argentine solution might have been procured in this way at the period we are considering, that I therefore hold such must have been the solution employed in Egypt; but that I merely suggest it as more probable and consistent with existing evidence, than the wholly gratuitous supposition that the marking-ink of ancient Egypt had nitrate of silver for its basis.

How the notion first arose, that the Israelitish idol was *dissolved,* I cannot comprehend, save that the text was never read

by a "solutionist," seeing that it is directly opposed to the plain meaning of the sacred narrative, which tells its tale in as clear, simple, and concise language as could be employed in the present day, were we desirous of relating the same facts in the most condensed form. These are the words :—"And he took the calf which they had made, and burnt it in the fire, and ground it to powder, and strawed it upon the water, and made the children of Israel drink of it." (Exodus, xxxii. 20.)   The other version of the translation closely resembles the foregoing :—"And I took your sin, the calf which ye had made, and burnt it with fire, and stamped it, and ground it very small, even until it was as small as dust ; and I cast the dust thereof into the brook that descended out of the mount." (Deut. ix. 21.)   Can anything be more evident than that the golden calf was reduced to an impalpable powder, and thus rendered potable when mixed with water ?   Yet Mr. Herapath, like many before him, writes,—"A probable speculation might be raised upon this" (the assumed knowledge of the uses of nitric acid by the Egyptians) "to account for the solution of the golden calf by Moses ;" and then, after destroying the Chimæra of the solution of the calf in sulphuret of potassium, tumbles himself into this Charybdis,—"It is therefore more probable that the priests had taught Moses the use of the mixed nitric and hydrochloric acids with which he could dissolve the statue, rather than a sulphuret, which we have no evidence of their being acquainted with," an observation which I have endeavoured to show is equally applicable to these two acids.

If it be asked, How did Moses grind this malleable idol "as fine as dust ?" the answer seems to me very easy ; in the words of the text, "he burnt it with fire ;" that is, he fused and alloyed it with a substance capable of rendering gold brittle.   What this was I pretend not to say, but many bodies possess this property ; it might have been arsenic, more probably antimony, but still more probably it was lead ; I say, still more probably, as, although we know the antiquity of the use of sulphuret of antimony for painting the eyes and eyebrows in the East, yet I am unaware of any positive evidence that it was known to the ancient Egyptians ; whilst with regard to lead, we have both material evidence and written testimony,—"Only the gold, and the silver, the brass, the iron, the tin, and the *lead*" (Numbers, xxxi. 12),—that lead was then a common metal ; whilst with respect to the properties of this alloy, L. Gmelin, vi. p. 245 (Cavendish Soc.), thus describes an "Alloy of gold and lead :—11 parts of gold and 1 part of lead form a pale yellow alloy, *as brittle as glass.*   The ductility of gold is destroyed by admixture of $\frac{1}{1920}$ of lead." Now without presuming to say that lead was actually the material used by Moses to render the golden calf so brittle as to

enable him to grind it "as fine as dust," yet I would submit, as this metal completely fulfills every condition required by the history, and as dokimasy was then sufficiently advanced to allow of such an alloy being made, that it assumes a very high degree of probability, being in complete and exact accordance both with the sacred narrative and also with the ascertained state of the metallurgic art at the time, that the golden calf was alloyed with lead; that this brittle alloy, when stamped and ground as fine as dust, was "strawed" on the water from the mount, of which the Israelites drank, and that a *solution* of the idol was neither effected nor even thought of.

<div align="right">I am, Gentlemen, yours, &c.</div>

Putney, July 19, 1852. <div align="right">J. Denham Smith.</div>

XXI. *Addenda to the Investigation on the Decennial Period in the Magnitude of the Daily Motion of the Magnetic Needle.* By Dr. Lamont[*].

IN the June Number of this Magazine a paper from me will be found, in which I have endeavoured to show that a *decennial period* exists in the daily motion of declination; at the conclusion of the said paper it is hinted, that in the horizontal intensity also a similar period probably exists. At that time the observations of our magnetic observatory were not so completely calculated as to permit of a closer discussion of the subject. At present, the calculations, at least so far as is necessary for a preliminary investigation, are carried out, and I will not neglect communicating the results.

As magnitude of the daily motion of the horizontal intensity, I assume, approximately, the difference between the position at 11 o'clock in the morning and 6 o'clock in the evening, and thereby obtain the following means for the years specified (expressed in ten-thousandths of the horizontal intensity) :—

1843.	.	.	.	.	7·8
1844.	.	.	.	.	6·9
1845.	.	.	.	.	6·6
1846.	.	.	.	.	11·4
1847.	.	.	.	.	12·1
1848.	.	.	.	.	14·3
1849.	.	.	.	.	12·0
1850.	.	.	.	.	10·7
1851.	.	.	.	.	9·1

Although no regular transition, as in the case of the declination, is to be observed here, still the existence of a period is very

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[*] From Poggendorff's *Annalen*, vol. lxxxvi. p 88.

plainly indicated. As the series embraces too few years to enable us to pronounce with certainty upon the duration and point of turning of the period, I will assume these to be the same as were found in the case of the declination. The magnitude of the motion will then be expressed by the formula

$$9 \cdot 82 + 3 \cdot 06 \sin (72° \cdot 58 + 34° \cdot 84 n),$$

where n expresses the number of years reckoned from **1848**.

From a comparison of the formula with the results of observation given above, the following differences are obtained :—

Year.	Difference. Calculation—Observation.
1843.	−1·0
1844.	−0·1
1845.	+1·6
1846.	−1·4
1847.	−0·4
1848.	−1·6
1849.	+0·7
1850.	+0·9
1851.	+0·8

The differences here are greater than in the case of the declination ; but I must remark in connexion with this, that I have not excluded the *days of disturbance*. These days exercise, however, an important influence, inasmuch as the causes of disturbance always operate in *the same sense,* and hence do not annul each other when the mean values are taken.

For the further establishment of the period of intensity, we shall look in vain to the observations of earlier times, some of which, in the case of the declination, we have found applicable, and nothing remains but to await the results of future observations.

For the present, the simple fact that the magnitude of the magnetic motions is subjected to a regular and very considerable increase and decrease, appears to me to imply consequences worthy of consideration ; for if, in the *effect,* a period be shown to exist, it must be the result of a corresponding period in the *influencing cause.* It is, however, quite certain, that in the temperature of the atmosphere—to which at present it is customary to refer the magnetic variations—no such period exists ; and for this reason I hold it to be absolutely necessary, either to give up totally the assumed influence of atmospheric temperature, or to modify it essentially by the introduction of a second coordinate cause.

XXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 11, "**O**N the Lunar Atmospheric Tide at Singapore." By
1852. Captain C. M. Elliot, M.E., F.R.S.

The discussion of the barometric observations at St. Helena by Colonel Sabine having clearly and decidedly shown the moon's influence on the atmosphere, the author determined to discuss in a similar manner the barometric observations at Singapore. The results of this discussion are given in the present communication.

In order that a comparison might be made between the results at Singapore and at St. Helena, he copied to a considerable extent the form of the different lunar tables drawn up by Colonel Sabine in his paper published in the Philosophical Transactions.

The observatory at Singapore was in latitude $1° 18' 32''$ N. and longitude $103° 56' 30''$ E. of Greenwich. The cistern of the barometer, one of Newman's, having a tube $0·532$ inch in diameter, was a few feet above high-water mark. The observations, during the whole of 1841 and the early part of 1842 and that of 1843, were made at every two hours; during the remainder of the time, to the close of 1845, at every hour.

The diurnal variation of the barometer having been eliminated, by deducting the mean monthly height at each hour, from the height given by observation, the residual quantities were arranged in tables; and the observation corresponding the nearest in time to the moon's superior culmination for each day being marked as 0 hour of lunar time, the whole were again rearranged in tables according to lunar hours. The variation or range of the mean of the sums of the differences thus arranged is exhibited in a table, in the last column of which are given the means of all the hours for each period of six months. In a second table are given the differences between these mean results in the last column of the preceding table and the numbers corresponding to the several hours in the other columns.

The means of the complete years of observation, 1841, 1844, 1845, are shown in a third table, in which are also given the means of the first six months of 1842 and 1843, during which two-hourly observations were made, and the means of the latter halves of these years, during which the observations were made hourly.

The means of the twenty-four months of the two-hourly observations, and of the thirty-six months of the hourly observations, are given in Table IV. Finally, Table V. exhibits the results of the observations of three years, so combined as to show the effect on the barometer, of the moon when similarly situated with reference both to its superior and inferior passage. In a column of this table are given the results of two years' observations at St. Helena, extracted from Colonel Sabine's paper. From a comparison, it appears that the effect produced by the moon on the barometer at Singapore, nearly on the equator, is slightly greater than at St. Helena, more distant from it by $14\frac{1}{2}°$ of latitude.

March 18.—A paper was read, entitled, " On the Blood-proper and Chylo-aqueous Fluid of Invertebrate Animals." By Thomas Williams, M.D.

In this paper the author has accumulated numerous observations, founded upon dissection and microscopic inquiry, to prove that there exist in *invertebrate animals* two distinct kinds of nutrient fluids ; that in some classes of this sub-kingdom these two fluids coexist in the same organism, though contained in distinct systems of conduits, while in others they become united into one. The author proposes to distinguish these two orders of fluids under the denominations of the *blood-proper* and *chylo-aqueous fluid*. The former is always contained in definitively organized (walled) blood-vessels, and having a determinate circulatory movement ; the latter, with equal constancy, in chambers and irregular cavities and cells, communicating invariably with the peritoneal space, having not a determinate circulation, but a to-and-fro movement, maintained by muscular and ciliary agency. He then adduces evidence, derived from dissection, in proof of the statement that the system of the blood-proper does not exist under any form, the most rudimentary, below the Echinodermata ; that, in other words, the system of the true blood, or of the blood-proper, begins at the Echinodermata. The author then shows that below the Echinodermata, namely in the families of Polypes and Acalephæ, the digestive and circulatory systems are identified, and that consequently the external medium is admitted directly into the nutrient fluids. He considers that this circumstance constitutes a fundamental distinction between the chylo-aqueous system and that of the blood-proper, into which, under no conditions, is the external inorganic element directly introduced.

He conceives that his observations suffice to establish the law, with reference to the chylo-aqueous fluid, that in every class in which it exists, it is charged more or less abundantly with *organized corpuscles*. This is an invariable fact in the history of this fluid. His inquiries show that these corpuscles are marked by distinctive microscopic characters, not in different classes and genera only, but in different species, entitling these bodies to great consideration in the establishment of species.

The paper then proceeds to demonstrate the proposition, that in those classes, as in the Echinodermata, Entozoa and Annelida, in which, in the adult animal, these two orders of fluids coexist, though distinct, in the same individual, there prevails between them, as respects their magnitude or development, an inverse proportion ; that while, as instanced in the Echinoderms, the chylo-aqueous fluid filling the ciliated space between the stomach and integument is considerable in volume, the blood-proper and *its* system are *little* evolved ; that while, as in the Entozoa, the chylo-aqueous fluid is still the most important fluid element in the organism, the blood system is proportionally rudimentary ; that in the Annelida, especially the higher species of that class, the chylo-aqueous fluid almost disappears, while the system of the true blood acquires, illustrating the law of inverse proportion, a correspondingly-augmented develop-

ment. The author then states, that the system of the chylo-aqueous fluid does not exist in the *adult*, but only in the *larva* state of the higher members of the articulated series, such as the Myriapoda, Insecta and Crustacea.

In Myriapods and Insects, he has observed that the peritoneal space is occupied by a fluid which does not communicate with, and is distinct in composition from, the contents of the true blood-vessels.

This peritoneal fluid, however, in these classes disappears at a subsequent stage of growth. Thus the author thinks that a continuous chain, *through the medium of the fluids*, is established between the Echinoderms at one extreme and the Crustacea at the other. These classes he proposes to connect together under the designation of the *double fluid series*, corresponding to the radiate and articulate series of systematic zoologists.

Returning to the standard of the Echinoderms, where the system of the blood-proper first appears in the zoological scale, he shows that at this point the *Molluscan* chain diverges from the radiate and articulate chain, and may be indicated, in contradistinction from the latter, as the *single-fluid series*. The author's observations lead him to believe, with Professor Milne-Edwards, that in all Molluscs, from the Tunicata to the Cephalopods, the chamber of the peritoneal is continuous with the channels of the circulation, and that consequently the fluids observed in these parts are *one and the same fluid*, establishing the *singleness* of the fluid system of the body ; and this conclusion is corroborated by additional evidence drawn from microscopic examinations.

He then recapitulates the results of his researches, and maintains that the base of the invertebrated kingdom of animals is formed of all those inferior series which rank below the Echinoderms ; and that this series is distinguished from the Molluscan, in which also the fluid system is single, by the important circumstance that in the former, unlike the Mollusca, the digestive and circulatory system are identified, or confounded into a single system ; that at the Echinoderms the series divaricates into the double-fluid series and single-fluid series, the former coinciding with the radiate and articulate class, and joining the Vertebrata through the Crustacea ; the latter running parallel with the Molluscan order, and connecting itself to the Vertebrata through the Cephalopods.

The fluids of the zoophytic series are invariably corpusculated, but the corpuscles cannot yet be reduced to any definite type of conformation. In the Medusan series these bodies become more definitively organized. The author then demonstrates, that throughout the whole radiate and articulate classes, wherever it is found, the *chylo-aqueous* fluid is richly corpusculated, or in other words, charged with floating morphotic elements, which, from the constancy of their characters in different species, become grounds for specific distinctions. It is stated, that, throughout the Echinoderms, Entozoa and Annelida, in which, even in the adult animal, the blood-proper and the chylo-aqueous fluid, though separate, coexist, the latter fluid only is corpusculated, the true blood being invariably

limpid and perfectly fluid (incorpusculated), and almost always the seat of the colour; the latter existing as a substance dissolved in the fluid, while in no instance does colour develope itself in the chylo-aqueous fluid.

The paper then shows, that at the point where the chylo-aqueous system disappears, namely at the Myriapods, the true blood becomes the vehicle of the corpuscles.

And lastly, the author adduces a great variety of observations in confirmation of the statement, that throughout the whole Molluscan series without exception, coinciding with his " *single-fluid series*," the fluids are richly charged with corpuscles.

The paper is accompanied by numerous illustrations, displaying the characters of the morphotic elements of the circulating fluids of the Invertebrata.

April 1.—A paper was also read, entitled, " On the Electro-chemical Polarity of Gases." By W. R. Grove, Esq., M.A., F.R.S., &c.

The author refers to the experiments of Faraday on dielectric induction, to those of Gassiot on the increase of electrical effects of tension, according as the chemical intensities of a voltaic battery are increased, and to other results, which, though supporting the view of a physico-polar state of gaseous substances intervening between oppositely electrified surfaces, have not hitherto shown any change in the arrangement of the gaseous particles dependent upon their chemical characteristics.

The electric or voltaic disruptive discharge has hitherto presented only one phenomenon which offers any analogy to electrolysis, viz. that observed by Mr. Gassiot and others, of the positive terminal being more intensely heated than the negative, when the voltaic discharge passes between metals. With the voltaic arc the effects of heat and the destruction of the terminals so interfere with any effects properly due to the transmission of the electric current, that it is next to impossible to eliminate the latter; on the other hand, with the electric spark from an ordinary machine, the quantity of matter acted on is too minute to give satisfactory evidence of the changes taking place. Mr. Grove sought an intermediate degree of electrical action, and by the aid of an apparatus of Ruhmkorf for producing a powerful secondary current, the results detailed in this paper were mainly obtained.

A polished silver plate is laid on the pump plate of a good airpump, and a metallic point is attached to the rod passing through a collar of leathers at the top of the receiver, the point being adjusted at from one-eighth to one-fourth of an inch distance from the plate. Caustic potash is kept suspended in the receiver, and a mixture of oxygen and hydrogen, or atmospheric air and hydrogen, allowed to enter it, and then attenuated until the barometer stands at half an inch; the discharges from the secondary coil are now made to pass between the point and the plate, when if the latter be positive it is oxidated, if negative the spot of oxide is reduced.

If there be excess of oxygen and little or no hydrogen, oxidation takes place, whether the plate be positive or negative, though in

different degrees; and if the gas be wholly or mainly hydrogen, reduction takes place whether the plate be positive or negative.

At certain intermediate states of mixture rings or zones of alternate oxidation and reduction are shown, quite distinguishable from the ordinary succession of colours of thin plates, and showing alternations or periods of interference of electrical action.

The author then gives the results of experiments with several other metals, of which bismuth was the only one he found to produce effects anything like equal to the silver, though other metals showed them in some degree.

He also varied the gas or gases employed, and details the results obtained with several gases; among them carbonic oxide is the most worthy of note, as with it effects are produced similar to those with the mixture of oxygen and hydrogen, viz. oxidation when the plate was positive, and reduction when it was negative.

The author's theory or mode of explaining the results is as follows. The discharges are successive, not continuous, and antecedent to each discharge the intervening gas is thrown into a state of chemical polarity, similar to that which takes place in an electrolyte anterior to electrolysis; by this means the positive terminal has in juxtaposition with it oxygen or an electro-negative gas; the discharge takes place, and by the superficial ignition the layer of oxygen combines with the metal in contact with it.

Conversely, when the oxidated surface is negative and in contact with an electro-positive gas, the heat of the discharge produces reduction. The fact of oxidation only taking place when air or oxygen alone are present, and reduction only when hydrogen is present, he considers irreconcilable with the effects being attributable to the discharge itself, or to their being regarded as analogous to electrolysis; while these phenomena are corroborative of the view he puts forth.

The author refers to the experiments of Priestley, Karsten and others, in which spots or marks have been shown to be produced by electrical discharge, but which do not otherwise bear upon the objects sought to be elucidated by this paper.

April 22.—The following papers were read :—

1. "On the Structure of the Stem of *Victoria regia.*" By Arthur Henfrey, F.L.S. &c. Communicated by Professor Edward Forbes, F.R.S.

The investigation of the anatomy of *Victoria regia* acquires its interest from the fact of the relations which have been pointed out to exist between the Nymphæaceæ and some of the undoubted Monocotyledonous families, especially also from the researches of M. Trécul on the anatomy of *Nuphar lutea*, which plant that author describes as having a stem of the Monocotyledonous type of structure. Through the unfortunate death of the plant of *Victoria regia*, which had flowered for some time in the gardens of the Royal Botanic Society of London, the author had an opportunity of examining the anatomy of its stem. It is an upright rhizome, with undeveloped internodes, growing by a single terminal bud, apparently perennially, and attaining considerable thickness; on the outside it bears the remains of the petioles and flower-stalks, which

separate by disarticulation, and their remains are found arranged in spiral lines upon the outside, so as to give the short, thick rhizome the aspect of a piece of a palm stem. As in *Nuphar*, the roots are produced in bundles at the bases of the petioles, and fall off successively upwards as the new ones are developed, leaving very conspicuous scars. The internal structure of the stem is quite Monocotyledonous in its character, presenting no trace of the arrangement of the vascular bundles into rings of wood, no true woody fibres, and no cambium layer. The vascular bundles, which are composed exclusively of spiral, annular and reticulated ducts surrounded by elongated parenchymatous cellular tissue, are isolated and arranged just as in Monocotyledons, such as the Palms; and the outer part of the stem exhibits a cortical parenchyma, much more like that of the herbaceous rhizomes of the rush-like plants, than any other known structure; it bears not the least resemblance to the bark of Dicotyledons. The results of the investigation show that *Victoria*, like *Nuphar*, has a stem of essentially Monocotyledonous structure. The paper was accompanied by drawings illustrating the general and microscopic anatomy of the stem.

2. "On the Meteorology of the English Lake District, including the results of Observations on the Fall of Rain at various heights, up to 3166 feet above the Sea-Level:" Fifth paper, for the year 1851. By John Fletcher Miller, Esq., F.R.S. &c.

The author states that the results for the past year do not seem to call for any particular remarks, and as it appears desirable, as a general rule, to defer all attempts at deduction until after the completion of the observations, the Tables for 1851 are presented, without many notes or comments, in continuation of the series which have previously appeared in the Transactions of the Society. The table for January, 1851, is given as an example of the daily fall of rain in the district during an excessively wet month, and also as showing the form of permanently registering the returns from the various stations, when sent in at the close of each month. He remarks that the quantity of 38·86 inches precipitated on "The Stye" in January 1851, is, he believes, without a parallel in the temperate zone.

3. "Formulization of Horary Observations presumed *à priori* to be nearly of a Periodic nature." By S. M. Drach, Esq., F.R.A.S., F.R.G.S. Communicated by Colonel Sabine,R.A.,Treas.,V.P.R.S.&c.

Referring to his former publications on the subject (Proceed. Roy. Soc. March 1842, Phil. Mag. 1842–51), the author empirically resolves the formula

$$ht = H + \Sigma A_i \sin it + \Sigma a_i \cos it = H + \Sigma R_i \sin (it + \psi_i),$$

h being the effect observed at the hour-angle t, thus obtaining from the 24 hourly observations all values up to $i=12$. This method giving the values of A_i, a_i, R for the different months, he believes that by it the law of change connected with the sun's motion in longitude and declination will be most readily deduced. The formula is exemplified by calculations and results of the diurnal variation of magnetic declination for each month at the various Colonial

Observatories, and also of the temperature at the Cape, St. Helena, Hobarton, Toronto, Greenwich, Leith, and Melville Island. The author infers *that the temperatures taken at six-hourly intervals give for their sum four times the mean temperature of the day, whatever be the commencing hour* ; and thus travellers and voyagers observing at 5^h, 11^h, 17^h and 23^h, will get the mean temperature of their position at 2 p.m. Hence, from the communications of the captains of Merchant-men, the Atlantic oceanic temperatures might be mapped in the course of a year, and the isothermal curves on this broad level surface be accurately laid down (see Journ. R. Geograph. Soc. ix. p. 369). Excepting at Melville Island, R_1 is the greatest coefficient, ψ_1 is nearly constant, and

$$H + \Sigma_1^4 R_i \sin (it + \psi_i) + \cos 8 t \cos 2 t (F \sin t + G \cos t)$$

will give the *yearly* formula : the homonymous hours are expressed by $H + \Sigma_1^2 R_i \sin (it + \psi_i)$ as in the oceanic tides nearly. At Melville Island, $\psi_3 = 45°$ nearly and R_3 is the greatest. The semester from midwinter to midsummer is also nearly expressed by

$$P + Q \sin \odot \text{ long. for } R_1.$$

Having obtained the empirical R and ψ, or A and a, any theoretic formula can be tested by the results.

XXIII. *Intelligence and Miscellaneous Articles.*

RESEARCHES ON THE SULPHURETS WHICH ARE DECOMPOSABLE BY WATER. BY E. FREMY.

THE object of this paper is to make known the production and principal properties of a class of sulphurets hitherto little examined, and the study of which is alike interesting to chemists and geologists, from the light which it throws on the formation of mineral waters.

When we consider the action of water on the sulphurets. we find that these compounds may be divided into three classes : the first comprises the sulphurets of the alkalies and of the alkaline earths which dissolve in water ; the second is formed of the insoluble sulphurets ; the third consists of the sulphurets of boron, silicon, magnesium and aluminium, which are decomposed by water : these latter are scarcely known, owing to their preparation having hitherto been accompanied with great difficulties. In order to a thorough investigation of all the questions which are connected with the decomposition of the sulphurets by water, I first sought for a method by which they might be easily prepared. This method I will now describe.

It is well known that sulphur exerts no action upon silica, boracic acid, magnesia and alumina. I imagined it might be possible to replace the oxygen in these substances by sulphur by the intervention of a second affinity, as that of carbon for oxygen. Such decompositions, produced by two affinities, are not rare in chemistry ; and in some yet unpublished experiments on the fluorides, I had observed that the sulphuret of carbon completely decomposed the fluoride of calcium mixed with silica, producing sulphuret of calcium. I was therefore led to presume that the sulphuret of carbon, acting by its two elements upon the preceding oxides, would remove the oxygen

by means of the carbon which it contains, and would at the same time form sulphurets: this supposition I found confirmed by experiment. In fact, I have obtained the sulphurets of boron, silicon, magnesium and aluminium, by submitting boracic acid, silica, magnesia and alumina, to the action of sulphuret of carbon at a high temperature. To facilitate the reaction, and remove the sulphuret from the decomposing action of the alkalies contained in the porcelain tubes, it is sometimes useful to mix the oxides to be reduced with charcoal, and to form them into little balls similar to those which are used in the preparation of chloride of silicon.

I have ascertained by analysis that these sulphurets correspond to the oxides from which they have been derived.

I will now say a few words of the sulphurets obtained by the above method. The sulphuret of silicon had been obtained in small quantity by Berzelius in the reaction of sulphur upon silicon, and by M. Pierre in the decomposition of chloride of silicon by hydrosulphuric acid. I have obtained this substance with the greatest ease, by passing the vapour of sulphuret of carbon over pellets of charcoal and gelatinous silica placed in a porcelain tube heated to bright red. The sulphuret of silicon condenses in the tube in beautiful white silky needles, which are not very volatile, but are readily carried along by the vapour.

To show the interest which attaches to the examination of this substance, it will suffice to mention here two of its reactions. When sulphuret of silicon is heated in a current of moist air, it is decomposed, and furnishes silky crystals of anhydrous silica; it is evident that we may explain by means of this experiment the natural production of certain filamentose crystals of silica. The sulphuret of silicon in the presence of water is decomposed with a brisk evolution of hydrosulphuric acid into silica, which remains entirely dissolved in the water, and is not deposited until the liquid is evaporated. It is impossible not to connect this curious property with those natural conditions under which certain mineral waters and siliceous incrustations are formed.

As the sulphuret of silicon is probably produced in all those cases where silica is submitted to the double action of a binary compound which cedes sulphur to it, and at the same time appropriates its oxygen, this sulphuret is probably not so rare as has been hitherto thought; and by admitting its presence in those rocks in which sulphurous springs occur, we might explain the simultaneous existence of silica and sulphuretted hydrogen in the principal sulphurous waters. This hypothesis is in some measure confirmed by the interesting observations of M. Descloizeaux, which show that the siliceous springs of the Geysers of Iceland contain a large quantity of sulphuretted hydrogen.

I content myself with submitting these considerations to geologists, merely observing that, in explaining the formation of sulphurous and siliceous waters by the decomposition of the sulphuret of silicon, I am only extending the ingenious theory proposed by M. Dumas to explain the formation of boracic acid.

The sulphurets of boron and aluminium were prepared like the sulphuret of silicon, and are likewise decomposed by water.

The sulphuret of magnesium I obtained by passing sulphuret of carbon over pure magnesia; in this case the presence of charcoal does not appear to be of any use. This sulphuret crystallizes, and is soluble in cold water; when its solution is kept at the ordinary temperature, there is but a feeble disengagement of sulphureted hydrogen; but when heated to ebullition, a lively effervescence of sulphureted hydrogen takes place, and there is an immediate deposition of magnesia.—*Comptes Rendus*, July 5, 1852.

ON THE EXISTENCE OF ORGANIC MATTER IN STALACTITES AND STALAGMITES, FORMING CRYSTALLIZED AND AMORPHOUS CRENATE OF LIME. BY DAVID A. WELLS.

In the eighth chapter of Liebig's Agricultural Chemistry, edited by Playfair, there is given the result of some examinations of stalactites from caverns in Germany, and from the vaults of old castles upon the Rhine, made with the view of ascertaining the fact of the presence or absence of organic matter in these bodies, either combined or uncombined.

The result may be stated in the words of the author, Prof. Liebig. The stalactites from the caverns "contain no trace of vegetable matter, and no humic acid, and may be heated to redness without becoming black." In the stalactites from the vaults and cellars of old castles, he says, "we could not detect the smallest traces" of humic acid. "There could scarcely be found a more clear and convincing proof of the absence of the humic acid of chemists in common vegetable mould." Under the term humic acid, Prof. Liebig undoubtedly means to include all those organic acids arising from the decomposition of vegetable matter, and which have received the names of crenic, apocrenic, geic and humic acids.

Having been informed by Dr. A. A. Hayes of Boston, that he had in numerous examinations arrived at results directly opposed to those of Prof. Liebig, I was induced at his suggestion to make an examination of a large number of stalactites and stalagmites obtained from various localities, with reference solely to the presence or absence of organic matter in these bodies.

The specimens examined were all from caverns, or rock formations, and were obtained from various parts of the United States, from Trieste in Austria, Malta and the Sandwich Islands. In colour they varied from an almost pure white to red, yellow, and brown of different shades; and in crystalline character, from a structure resembling arragonite to a variety entirely wanting in symmetrical arrangement, or a mere incrustation. The specimens were dissolved in dilute hydrochloric acid, the flocculent matter separated, collected and washed, boiled in caustic potash, carbonate of ammonia or carbonate of soda, and then tested in the usual way for crenic and apocrenic acids by acetate of copper and carbonate of ammonia. In all the varieties, with one exception, abundant flocculent organic matter was separated, which on testing gave evidence of crenic acid in considerable quantities, with doubtful traces of apocrenic acid. The exception alluded to was the specimen examined from Trieste, which did not afford any appreciable flocculent matter on dissolving

in acid. The greatest quantity of organic matter was found in stalactites of a deep yellow colour, highly crystalline and uniform in character, and in the portions examined perfectly homogeneous and free from layers, or intervening bands indicating different periods and changes in deposition. As the presence of iron could not be found in the acid solution, it is inferred that the colour of these yellow stalactites must be owing in great part to combined organic matter, existing as crenate of lime. In specimens like the spar ornaments from the Rock of Gibraltar, with which all are familiar, the colouring and delicate shading is also probably due to organic matter.

Dr. Hayes informs me, that he has also found organic matter in arragonite in sufficient quantity to separate in flakes, while the specimen was dissolving in acid.

From these statements, it must, I think, be inferred, contrary to the view of Liebig, that organic matter does exist in stalactites generally, as an acid combined with the lime, and imparting to them their various colours. I would by no means call in question the accuracy of the experiments of Prof. Liebig, further than that, as far as my observations extend, crenic acid in the presence of lime, and combined with it, passes over like oxalates, upon heating, into carbonates, without perceptible blackening.

It may here be added, that Prof. Johnston of England describes a compound of alumina with crenic acid, occurring in caves of granite upon the coast of Cornwall. This mineral has received the name of Pigotite, and is observed in places where the surface-water trickles down over the granite rocks. From this it may not be inappropriate to apply the term *crenite* to those lime formations in which crenic acid occurs in considerable quantities.

Results similar to those announced above have been obtained by Dr. C. T. Jackson, as well as by Dr. Hayes of Boston. Dr. J. Lawrence Smith informs me, that he has frequently met with crenic acid in lime concretions from Asia Minor, and its existence in stalactites was also announced by Dr. Emmons of Albany some years since. My results can therefore be considered but as the verification of those obtained by others.—Silliman's *Journal*, Jan. 1852.

ON THE NEW METAL DONARIUM.

A few months ago M. Bergemann discovered an oxide in a mineral from Langesundfiord, near Brevig in Norway, which he considered to be new. He gave the name Donarium to the metal, and that of Orangite to the mineral*.

Damour has since examined a specimen of orangite. Its specific gravity was 5·19; Bergemann found 5·39. On comparing his analysis with that of Bergemann, and also the properties of the supposed new oxide, M. Damour concludes that the oxide of donarium is nothing less than impure thorina. Bergemann's analysis does not enumerate oxide of lead and oxide of uranium among the constituents. M. Berlin of Lund has also found that the oxide of donarium is thorina mixed with minute traces of oxide of uranium, oxide of

* See pp. 583 and 390 of vols. i. and ii. of the present Series.

iron, vanadic acid, tin, and perhaps a little molybdic acid. The following are the analyses :—

Damour.		Berlin.	
Silica	17·52	Silica..............	17·78
Thorina	71·65	Thorina............	73·29
Lime................	1·59	Lime	0·92
Oxide of lead	0·88	Oxide of uranium ..	
Oxide of uranium	1·13	Peroxide of iron,....	0·96
Oxide of manganese ..	0·28	Tin	
Peroxide of iron	0·31	Vanadium	
Magnesia............	trace	Water	7·12
Alumina	0·17		100·00
Potash	0·14		
Soda................	0·33		
Water, with trace of carbonic acid	6·14		
	100·14		

Damour deduces from his analysis the formula $3ThO + SiO^3 + 2HO$. Berzelius assumed that thorite consisted of several silicates, but principally of a silicate of thorina of the formula $3ThO + SiO^3 + 2HO$. Damour is of opinion that Berzelius's analyses do not lead to any definite proportion; but they prove that orangite and thorite are identical, and that the metal donarium must be struck from the list of simple bodies.

Berlin also calculates from his analysis the formula

$$3ThO + SiO^3 + 2HO,$$

and is likewise of opinion that orangite is only a purer thorite. He also draws attention to a peculiar property of thorina. It is stated that calcined thorina is insoluble in acids. This is correct as far as regards the earth obtained by calcining the hydrate, but not for that obtained by igniting the oxalate, which dissolves slowly in hydrochloric acid.—*Central Blatt*, June 23, 1852.

ON A NEW MODE OF MEASURING HIGH TEMPERATURES.
BY MR. JOHN WILSON.

After referring to, and describing briefly the pyrometers at present in use, the paper explained the method employed by the author to measure high temperatures. According to his plan, a given weight of platinum is exposed for a few minutes to the fire, the temperature of which is required to be measured, and then plunged into a vessel containing water of a determined weight and temperature. After the heat of the platinum has been communicated to the water, the temperature of the water is ascertained; and from this is estimated the temperature to which the platinum was subjected. Thus, if the piece of platinum employed be 1000 grains, and the water into which it is plunged be 2000 grains, and its temperature 60°, should the heated platinum when dropped into the water raise its temperature to 90°, then 90°—60°=30°; which, multiplied by 2 (because the water is twice the weight of the platinum), gives 60°, that an equal weight of water would have been raised. Again; should the water in another case gain 40°, then

$40° \times 2 = 80°$, the temperature measured by the pyrometer. To convert the degrees of this instrument into degrees of Fahrenheit, we must multiply by 31·25, or $31\frac{1}{4}$. Thus, $80° \times 31\frac{1}{4}$ would give 2500° of Fahrenheit. And $60° \times 31\frac{1}{4} = 1875°$. The multiplier 31·25 is the number expressing the specific heat of water as compared with that of platinum, the latter being regarded as 1.

In order to attain very accurate results by this method, precautions similar to those required in determining the specific heat of bodies must be taken ; that is, it is necessary to guard against the dissipation of heat by conduction and radiation. The apparatus used by the author consists of a polished tinned iron vessel, of a cylindrical form, 3 inches deep and 2 inches in diameter; this is placed within a concentric cylinder, separated from the enclosed vessel about $\frac{1}{4}$ inch. By this means there is but little heat lost during the experiment, either by radiation or conduction.

At the commencement of the experiments, the author imagined it would be necessary to employ a considerable proportion of water, and therefore took twenty-five times the weight of the platinum ; but he found that the temperature gained by the water, even in cases of very high heats, did not exceed 4° or 5° ; and an error of 1°, when converted into degrees of Fahrenheit, amounted to 400°. To obtain results within much narrower limits of error, it became obvious, a much smaller proportion of water should be employed ; and ultimately it was found that double the weight of the platinum was in all cases sufficient.

There is no appreciable loss of heat from the evaporation of steam when the hot platinum is plunged into the water ;—there is probably no actual contact with the water until the platinum is fairly at the bottom of the water. It is in fact the converse of dropping water on a plate of platinum or iron strongly heated ; in which case the water, instead of being suddenly dissipated as steam, assumes the spheroidal form, and runs about over the plate without coming in contact with the heated surface. It is only when the temperature of the metal becomes much reduced that the water is rapidly converted into vapour.

In ascertaining temperatures by this pyrometer, a correction has to be made for the portion of the total heat that is absorbed by

        1st, the mercury of the thermometer in the water ;

        2nd, the glass bulb and stem of the thermometer ;

        3rd, the iron vessel containing the water ;

        4th, the heat retained by the piece of platinum.

The portion of the total heat that is absorbed by these several bodies, compared to the portion received by the water, will be in proportion to their several weights, and the specific heat of each compared with water.

					Equivalent grs. of water.
Mercury ...	200 grains	$\times \frac{1}{30}$th specific heat=			7
Glass	35	,,	$\times \frac{1}{6}$th	,,	6
Iron	658	,,	$\times \frac{1}{9}$th	,,	73
Platinum ...	1000	,,	$\times \frac{1}{32}$nd	,,	31
			Total	....	117

Therefore the effect of these bodies is equivalent to the addition of 117 grains to the 2000 grains of water, or $\frac{1}{17}$th has to be added as a correction to all the temperatures obtained by this instrument; or, in other words, the multiplier must be increased from $31\frac{1}{4}$ to 33 in this instrument, and in all similar ones where the weights of the mercury and glass of the thermometer, and of the iron vessel, are the same as stated above.

As the piece of platinum is the most expensive part of the apparatus, it is proposed to use a small piece of baked Stourbridge clay as a substitute for the platinum. The author has found, by experiment, that a piece of Stourbridge clay, 200 grains in weight, when heated to the melting-point of silver, and plunged into the tinned vessel containing 2000 grains of water, raises the temperature of the water 41°.

Now, if 1890° Fahrenheit (the melting-point of silver) be divided by 41, we obtain 46° as the number corresponding to 1° of this pyrometer; and 46 will therefore be the correct multiplier; and no corrections are required for any heat abstracted by the thermometer, the tinned vessel, or the piece of clay.

The temperature of all sorts of furnaces and flues of steam-engines, &c., may be readily ascertained by means of the piece of Stourbridge clay.—*Proceedings of the Institution of Mechanical Engineers, Birmingham.*

METEOROLOGICAL OBSERVATIONS FOR JUNE 1852.

Chiswick.—June 1. Clear and fine. 2. Cloudy: fine: rain. 3. Cloudy. 4. Overcast: fine: clear. 5. Very fine: slight rain. 6. Rain: clear at night. 7. Constant rain. 8. Thick whitish haze: low fog in the evening: heavy rain. 9. Excessively heavy rain throughout. 10. Rain: cloudy: clear. 11. Overcast. 12. Slight rain: overcast. 13. Fine: rain at night. 14. Showery. 15. Fine: showery: clear. 16. Rain: uniformly overcast. 17. Cloudy throughout. 18. Rain: showery: heavy rain. 19. Heavy clouds: clear and fine. 20. Overcast: rain. 21. Rain: cloudy. 22—24. Fine. 25. Uniformly overcast: fine: rain at night. 26. Rain: heavy showers. 27. Overcast: heavy showers. 28. Fine: densely overcast. 29. Overcast: cloudy: clear. 30. Fine: rather windy: clear at night.—More rain fell on the 7th, 8th, and 9th, than on any three consecutive days for at least twenty-six years near London.

Mean temperature of the month	58°·01
Mean temperature of June 1851	59·21
Mean temperature of June for the last twenty-six years	60·61
Average amount of rain in June	1·77 inch.

Boston.—June 1. Fine. 2. Fine: rain P.M. 3. Cloudy: rain A.M. 4. Fine: rain A.M. 5. Fine. 6. Rain: rain A.M. 7, 8. Cloudy. 9. Cloudy: rain A.M: 10, 11. Cloudy: rain A.M. and P.M. 12. Cloudy: rain A.M. 13. Cloudy: rain P.M. 14, 15. Cloudy: rain A.M. and P.M. 16. Rain: rain A.M. and P.M. 17. Fine. rain A.M. and P.M. 18, 19. Cloudy: rain A.M. 20. Cloudy: rain P.M. 21. Rain: rain A.M. 22. Cloudy: rain A.M. and P.M. 23—25. Fine. 26. Cloudy: rain A.M.: 27. Cloudy. 28. Cloudy: rain P.M. 29, 30. Cloudy.

Sandwick Manse, Orkney.—June 1, 2. Showers. 3. Rain: showers. 4, 5. Bright: clear: fine. 6. Hazy. 7. Hazy: clear: fine. 8. Bright: fine. 9. Cloudy: damp. 10. Drizzle. 11. Drizzle: showers. 12. Damp: bright. 13. Clear: fine: cloudy. 14. Showers: cloudy: fine. 15. Bright: fine: clear: fine. 16. Bright: fine: cloudy. 17. Clear: fine: cloudy. 18. Damp: fog. 19. Bright: clear: fine. 20. Damp. 21. Damp: fog. 22, 23. Rain. 24, 25. Bright: showers. 26. Bright: rain. 27. Bright: showers: fine. 28. Clear: fine: drops: fine. 29. Clear: fine: cloudy: fine. 30. Rain.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1852. June.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8¼ a.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. Boston. 8½ a.m.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Orkney, Sandwick.	Rain. Chiswick.	Rain. Boston.	Rain. Orkney, Sandwick.
1. O	29·889	29·849	29·40	29·64	29·65	52·5	66	48	47	46	w.	w.	e.	·07		·16
2.	29·866	29·791	29·35	29·63	29·63	60	63	50	57	48½	sw.	s.	e.	·09		·18
3.	29·838	29·709	29·16	29·54	29·46	62	65	41	51	49½	sw.	ssw.	se.	·03	·40	·48
4.	29·901	29·716	29·26	29·66	29·78	57·5	70	43	54	54	sw.	ssw.	se.	·02	·01	·12
5.	29·970	29·873	29·46	29·87	29·96	57·5	69	54	56	57	s.	e.	ese.	·03		
6.	29·839	29·668	29·27	29·96	29·94	61·5	71	52	56	51	sw.	se.	e.	·03		
7.	29·579	29·539	29·13	29·96	30·01	67	62	51	55½	51	se.	e.	ese.	·41	·32	
8.	29·617	29·580	29·13	30·01	29·98	66	69	51	60	55½	se.	calm	ese.	·45		
9. ☾	29·570	29·518	29·10	29·91	29·77	59	61	51	54	55	sw.	n.	n.	1·48		
10.	29·548	29·539	29·05	29·64	29·59	51	59	42	50	49½	sw.	n.	n.	·18	·12	·04
11.	29·598	29·490	28·96	29·54	29·57	49·5	57	40	53½	51	nw.	wnw.	nnw.	·01	·15	
12.	29·738	29·589	29·17	29·64	29·63	47·5	61	42	53½	50	w.	n.	w.	·02	·35	
13.	29·719	29·476	29·20	29·52	29·40	46·5	65	48	53½	50	nw.	wnw.	ese.	·28	·13	
14.	29·374	29·214	28·73	29·33	29·42	60	67	46	54	53	w.	wnw.	e.	·13		
15.	29·570	29·477	28·99	29·53	29·56	60	67	44	55½	53	w.	w.	ese.	·16	·12	·07
16.	29·415	29·384	28·94	29·50	29·41	57·5	68	50	58	55	sw.	se.	e.	·20	·35	
17. ●	29·490	29·437	29·00	29·41	29·50	60	67	50	59	55	s.	s.	e.	·09	·12	·07
18.	29·538	29·538	29·08	29·61	29·70	63	66	52	59	56	s.	s.	ese.	·30	·07	
19.	29·730	29·578	29·14	29·76	29·80	67	69	49	63	55	sw.	s.	e.	·12	·13	
20.	29·577	29·577	29·30	29·69	29·58	61	68	56	59	55	s.	ssw.	w.	·05	·09	
21.	29·622	29·541	29·12	29·48	29·46	65	68	46	58½	53	sw.	ssw.	ese.	·02	·36	
22.	29·745	29·628	29·14	29·26	29·28	62	68	51	57½	53	sw.	ssw.	sse.		·09	
23.	29·849	29·717	29·23	29·36	29·53	59	72	47	57½	58	w.	ssw.	sw.		·14	·07
24. ☽	30·075	29·986	29·46	29·70	29·82	63	76	44	60	57½	s.	s.	se.	·01		·09
25.	30·048	29·904	29·54	29·85	29·75	61	74	57	61	60	sw.	ssw.	se.	·32		·65
26.	29·814	29·708	29·25	29·67	29·62	63·5	71	52	60	55½	sw.	s.	ese.	·12		·22
27.	29·824	29·760	29·30	29·56	29·65	66	71	51	62	56½	sw.	ssw.	w.	·04	·17	·04
28.	29·786	29·761	29·24	29·52	29·59	63	68	56	62	56	sw.	s.	ese.	·03		·02
29.	29·787	29·715	29·23	29·52	29·33	67	67	53	62	59	sw.	sw.	w.		·27	·22
30.	29·937	29·844	29·20	29·28	29·41	64	71	47	58½	58½	sw.	w.	w.			·03
Mean.	29·737	29·637	29·18	29·621	29·622	60·0	67·23	48·80	56·95	53·71				4·69	3·39	3·05

XXIV. *On some Salts and Products of Decomposition of Pyromeconic Acid. By* Mr. JAMES F. BROWN[*].

THIS acid was discovered by Sertuerner, and long viewed as sublimed meconic acid, till Robiquet in 1832 obtained the meconic acid from which it is produced, and showed that the acid existing in opium differed in its properties from the sublimed acid: he also prepared and analysed its lead salt, from which he deduced the formula PbO, $C^{10} H^3 O^5$. Liebig has observed that its composition is the same as that of pyromucic acid, and thought it probable these acids might prove identical. This assertion, however, has been refuted by Dr. Stenhouse in his paper on the subject, to some of the details in which I shall have occasion to refer.

The pyromeconic acid employed in the following experiments was obtained by distilling, at a temperature of about 500° and 600° F., the impure meconic acid got by treating the crude meconate of lime twice with hydrochloric acid, which, though much coloured, answered sufficiently well. For the purification of the acid, which when first sublimed is in the form of an oily semifused mass, Stenhouse recommends pressure between folds of filtering paper, redistillation, and frequent crystallization from boiling alcohol. I found, however, that simple pressure, and sublimation at a comparatively low temperature in a cylindrical glass vessel provided with a number of diaphragms of filtering paper, rendered the acid perfectly colourless, and pure enough for the preparation of all its salts and products of decomposition.

As thus obtained, it is in beautiful large transparent plates, of ready solubility in water and alcohol, both hot and cold, from

[*] Communicated by the Author, having been read before the Royal Society of Edinburgh, March 1, 1852.

which it crystallizes in four-sided prisms of considerable size. It is slightly acid to litmus, and even after three crystallizations from boiling water it retained its acidity. It is completely volatile at 212°. A quantity having been exposed to that temperature for about fourteen hours, was found to have entirely disappeared. This property may serve as a test of its purity from paracomenic acid, with which pyromeconic, as first sublimed, is always contaminated, that acid requiring a much higher heat to volatilize it. It gives, as is well known, a deep red colour with persalts of iron, and does not precipitate chloride of calcium, barium, manganese, nor sulphate of magnesia, either hot or cold, even on the addition of a small quantity of ammonia. Bichloride of mercury gives after some time a white amorphous precipitate, soluble on boiling the fluid. When a hot aqueous solution of pyromeconic acid is treated with strong caustic potash in excess, and allowed to stand some hours, crystals soon begin to form, which upon examination proved to be the acid unaltered; a similar experiment was made with ammonia, but with the same result, the fluids in both cases becoming nearly black.

To ascertain the purity of the acid, the following analysis was made of it, dried *in vacuo*, after one sublimation.

5·74 grains substance gave 11·133 carbonic acid and 1·905 water.

	Experiment.	Calculation.		
Carbon . .	53·23	53·57	C^{10}	60
Hydrogen .	3·71	3·57	H^4	4
Oxygen . .	43·06	42·86	O^6	48
	100·00	100·00		112

The formula of the acid is therefore represented by

$$C^{10} H^3 O^5 + HO.$$

Pyromeconate of Baryta.—This salt may be obtained by mixing a warm ammoniacal solution of pyromeconic acid with acetate of baryta, when it makes its appearance after a short time in small colourless silky needles. In dilute solutions they do not appear immediately, but after standing some time they commence forming and rapidly increase. It is the most soluble in water of all the earthy salts of this acid,—181·90 grains of a saturated solution at 60° F. gave on evaporation at 212° a residue of 4·50 grains = 2·50 per cent. It is of sparing solubility in alcohol. Like the other pyromeconates, it reacts strongly alkaline, and gives a slight red colour with chloride of iron, which may be made much more apparent if the crystals be employed instead of a solution of the salt. By evaporation *in vacuo* it deposits itself in short prisms of a yellowish colour. When ex-

posed to a temperature of 212°, it loses no weight; but heated to a higher temperature, it burns with a slight deflagration without previous fusion. The following are the results of analysis after the salt had been thoroughly washed with alcohol, and dried at 212°. The other salts of this acid were also dried at 212° previous to analysis.

4·81 grains substance gave 5·55 carbonic acid and 1·06 water.
4·33 grains substance gave 2·24 carbonate of baryta.

		Experiment.	Calculation.		
Carbon	.	. 31·46	31·82	C^{10}	60·00
Hydrogen	.	2·43	2·12	H^4	4·00
Oxygen	.	. 25·56	25·47	O^6	48·00
Baryta	.	. 40·55	40·59	BaO	76·55
		100·00	100·00		188·55

The composition of the salt is therefore represented by the formula BaO, $C^{10} H^3 O^5$ + HO.

Pyromeconate of Strontia.—When alcoholic solutions of nitrate of strontia and pyromeconic acid, made ammoniacal, are mixed, there immediately ensues a precipitate of minute silky needles, which by solution in water are obtained in stellar groups of a yellowish colour. As precipitated, it is colourless, sparingly soluble in cold water and alcohol, more so in hot, and reacts strongly alkaline. 224 grains aqueous solution at 68° gave 3·00 grains on evaporation at 212° = 1·3 per cent. It loses nothing at 212°, and at a higher temperature is infusible, but burns with a slight explosion.

The well-washed substance gave the following results on analysis, the strontia being determined as carbonate.

5·875 grains substance gave 7·79 carbonic acid and 1·35 water.
7·97 grains substance gave 3·58 carbonate of strontia.

		Experiment.	Calculation.		
Carbon	.	. 36·16	36·63	C^{10}	60
Hydrogen	.	2·74	2·44	H^4	4
Oxygen	.	. 29·61	29·31	O^6	48
Strontia	.	. 31·49	31·62	SrO	51·78
		100·00	100·00		163·78

The formula of the salt is therefore SrO, $C^{10} H^3 O^5$ + HO.

Pyromeconate of Lime.—This salt is obtained in the form of small colourless silky needles when a warm ammoniacal solution of pyromeconic acid is added to acetate of lime in excess. It is slightly soluble in boiling alcohol, but rather more so in water, from which it deposits itself by gradual cooling of the solution

in crystals of considerable size.   3·41 grains aqueous solution of this salt at 60° gave 1·08 grain of residue at 212°=0·31 per cent.

6·15 grains substance gave 10·26 carbonic acid and 1·56 water. 6·34 grains substance gave 2·34 carbonate of lime.

	Experiment.	Calculation.		
Carbon . .	42·94	42·85	C^{10}	60
Hydrogen .	2·60	2·85	H^4	4
Oxygen . .	34·02	34·30	O^6	48
Lime . . .	20·44	20·00	CaO	28
	100·00	100·00		140·00

Hence the composition of the salt is represented by the formula CaO, $C^{10} H^3 O^5$ + HO.

Pyromeconate of Magnesia.—A warm aqueous solution of pyromeconic acid gives with acetate of magnesia a white amorphous precipitate, insoluble in water and alcohol.   In its properties it closely resembles the other pyromeconates.   The following is the analysis of the salt, the magnesia being determined by ignition of it, and weighing the residue.   It loses nothing at 212°.

4·925 grains substance gave 8·735 carbonic acid and 1·235 water.   4·595 grains substance gave ·76 magnesia.

	Experiment.	Calculation.		
Carbon . .	48·37	48·51	C^{10}	60
Hydrogen .	2·76	2·41	H^3	3
Oxygen . .	32·34	32·37	O^5	40
Magnesia .	16·53	16·71	MgO	20·67
	100·00	100·00		123·67

From which it appears that this is the only earthy salt of pyromeconic acid which is anhydrous, the composition of it being expressed by the formula MgO, $C^{10} H^3 O^5$.

Pyromeconate of Lead.—When a warm concentrated solution of pyromeconic acid, made ammoniacal, is added to acetate of lead, it causes an immediate precipitate of a dense crystalline powder, which rapidly increases upon violent agitation of the fluid.   This salt, as has already been mentioned, was prepared and analysed by Robiquet, who formed it by adding hydrated oxide of lead to a hot solution of pyromeconic acid; he found it to be anhydrous, and to consist of PbO, $C^{10} H^3 O^5$.

The crystals require a considerable quantity of hot water for their solution; they are not so soluble in alcohol either hot or cold.   It is colourless when thrown down, but rapidly becomes yellow by exposure for any length of time to bright daylight.

It loses nothing at 212°, even after being kept for three or four hours at that temperature.

The following is the result of analysis, the lead being determined as sulphate by ignition of the salt with a few drops of strong sulphuric acid.

5·48 grains substance gave 5·65 carbonic acid and 0·815 water. 5·29 grains substance gave 3·74 sulphate of lead.

	Experiment.	Calculation.		
Carbon . . .	28·12	27·94	C^{10}	60
Hydrogen . .	1·65	1·39	H^3	3
Oxygen . . .	18·21	18·77	O^5	40
Oxide of lead .	52·02	51·90	PbO	111·56
	100·00	100·00		214·56

The formula is therefore represented by PbO, $C^{10} H^3 O^5$.

Pyromeconate of Copper.—The ammonio-sulphate of copper mixed with a warm aqueous solution of pyromeconic acid causes an immediate precipitate of this salt in bright green crystalline needles, which are extremely brittle and easily pulverized. This salt has also been examined by Stenhouse[*], who prepared it by boiling the acid with hydrated oxide of copper, and allowing the filtered solution to cool. The crystals require a considerable amount of hot water for their solution, and are very slightly soluble in cold water or alcohol.

The copper was determined as oxide by heating the salt to redness, and then igniting the residue with nitric acid. It loses nothing at 212°.

6·00 grains substance gave 1·66 oxide of copper, which calculated per-centage gives 27·66 of oxide of copper, the number 27·79 being that corresponding with the formula

$$CuO, C^{10} H^3 O^5.$$
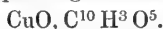
Pyromeconate of Iron.—Dr. Stenhouse has observed in the paper before mentioned, that when pyromeconic acid is boiled with hydrated peroxide of iron, and also with the persulphate, it combines with the oxide and forms a brownish-red powder, which when neutral is very little soluble in cold water. It may also be obtained, and perhaps more conveniently, by adding perchloride of iron to a hot concentrated solution of the acid in water, when the cinnabar-red crystals begin to make their appearance, adhering firmly to the sides and bottom of the vessel.

Its properties have been so fully described by Stenhouse, that further remark is unnecessary.

The crystals were well washed with cold water, until the fluid

* Mem. and Proc. Chem. Soc. vol. ii. [Phil. Mag. S. 3. vol. xxiv. p. 128.]

which passed through ceased to precipitate nitrate of silver, and then subjected to analysis in the usual way.

6·53 grains substance gave 1·32 peroxide of iron.

The per-centage calculated from the number is 20·21, while 20·56 is that corresponding to the formula $Fe^2 O^3, 3C^{10} H^3 O^5$.

Products of decomposition of Pyromeconic Acid.—If some crystals of the acid be moistened with strong nitric acid in the cold, they immediately assume a white gelatinous appearance, and bubbles of nitrous acid are soon evolved; by the application of a gentle heat the action becomes excessively violent, and continues so even though the heat be withdrawn, with the production of oxalic and hydrocyanic acids. Sulphuric acid in the cold has no action on pyromeconic acid; but when gently warmed, it dissolves it to a colourless fluid, which upon cooling deposits the pyromeconic acid again.

Several experiments were made by passing chlorine into a solution of pyromeconic acid with the view of obtaining a chlorine substitution compound, but without success, that reagent acting too powerfully on it. Complete decomposition always ensued, and oxalic acid was detected in the fluid, but not in large quantity. I may here mention that I failed in obtaining an æther of pyromeconic acid by passing dry hydrochloric acid gas into a solution of the acid in absolute alcohol, the crystals which deposited from the fluid proving on analysis to be the acid unaltered.

Action of Bromine on Pyromeconic Acid.—When bromine water is added to a strong aqueous solution of pyromeconic acid, leaving the latter in excess, it is rapidly absorbed, yielding a colourless fluid, which after standing for an hour or even less, deposits the new acid in beautiful small colourless prisms. In one experiment a large excess of bromine failed to yield any of the new acid, even after standing a considerable time; more bromine was then added, but no crystals made their appearance. The solution, which had acquired a yellowish colour, was evaporated to a small bulk, still without the formation of any bromo-pyromeconic acid. It had now become perfectly black, and oxalic acid was found in fluid.

The crystals obtained by the action of a limited quantity of bromine, after thorough washing with cold water, gave the following results on analysis:—

4·97 grains substance gave 5·82 carbonic acid and 0·925 water. 4·845 grains substance gave, when burned with lime, 4·71 grs. bromide of silver.

	Experiment.	Calculation.		
Carbon . .	31·93	31·70	C^{10}	60
Hydrogen .	2·05	1·58	H^3	3
Oxygen . .	24·68	25·65	O^6	48
Bromine .	41·34	41·07	Br	78·26
	100·00	100·00		189·26

Which show that they consist of an acid produced by the substitution of an equivalent of hydrogen in pyromeconic acid by an equivalent of bromine.

This acid is of sparing solubility in cold water, more so in hot, and reddens litmus slightly. Boiling alcohol dissolves it readily, from which it crystallizes in beautiful fibrous plates; and if the cooling be carefully effected, in short prisms. It imparts to persalts of iron a deep purple colour, quite distinct from the red produced by the original acid. Nitric acid decomposes it with effervescence, but sulphuric dissolves it without any apparent decomposition. Submitted to destructive distillation, it fuses and then blackens, hydrobromic acid being evolved in large quantity; after the continuation of the heat, a white crystalline substance begins to collect on the cool part of the tube, but in quantity too small to admit of examination.

It gives no precipitate with nitrate of silver, neither when boiled does it reduce the oxide to the metallic state. It causes no precipitate in solutions of chloride of barium, calcium, or sulphate of magnesia, even in the presence of ammonia. Ammonio-sulphate of copper, though producing no effect in the cold, gives when heated a precipitate of a bluish tint.

Bromo-pyromeconic acid, like pyromeconic, is monobasic, forming only one series of salts. The salt employed for controlling the analysis and establishing the saturating power of the acid was that of lead.

Bromo-pyromeconate of Lead.—A warm alcoholic solution of the acid gives with acetate of lead, also dissolved in alcohol, a white precipitate of small dense crystalline needles which rapidly fall to the bottom of the vessel. From its insolubility in water and alcohol, recrystallization of the salt was impossible; thorough washing with alcohol was therefore resorted to. This salt may also be obtained by using aqueous solutions of the acid and salt of lead with the addition of ammonia, but the product is in this case very much coloured.

The lead in the following analysis was determined as sulphate by ignition of the salt with sulphuric acid. It loses nothing at 212°.

7·34 grains substance gave 5·54 carbonic acid and 0·56 water.
4·68 grains substance gave 2·29 sulphate of lead.

	Experiment.	Calculation.		
Carbon . . .	20·57	19·91	C^{10}	60
Hydrogen . .	00·85	00·99	H^3	3
Oxygen . . .	...	16·19	O^6	48
Bromine . . .	...	26·01	Br	78·26
Oxide of lead .	36·48	36·90	PbO	111·56
	100·00	100·00		300·82

The composition of this salt is therefore expressed by the formula $PbO, C^{10} H^2 Br O^5 + HO$.

I have also obtained a substitution product of iodine, by operating in a particular manner, which I shall describe in detail in a future paper. I shall conclude this communication with a list of the substances described in it along with their formulæ.

These experiments were performed in the laboratory of Dr. Anderson, to whom I am much indebted for assistance.

Pyromeconic acid . . .	$C^{10} H^3 O^5 + HO.$
Pyromeconate of baryta .	$BaO, C^{10} H^3 O^5 + HO.$
Pyromeconate of strontia .	$SrO, C^{10} H^3 O^5 + HO.$
Pyromeconate of lime . .	$CaO, C^{10} H^3 O^5 + HO.$
Pyromeconate of magnesia .	$MgO, C^{10} H^3 O^5.$
Pyromeconate of lead . .	$PbO, C^{10} H^3 O^5.$
Pyromeconate of copper .	$CuO, C^{10} H^3 O^5.$
Pyromeconate of iron . .	$Fe^2 O^3, 3C^{10} H^3 O^5.$
Bromo-pyromeconic acid .	$C^{10} H^2 Br O^5 + HO.$
Lead salt	$PbO, C^{10} H^2 Br O^5 + HO.$

Edinburgh, July 1, 1852.

XXV. *On the Dynamical Theory of Heat, with numerical results deduced from* Mr. Joule's *equivalent of a Thermal Unit, and* M. Regnault's *Observations on Steam.* By WILLIAM THOMSON, *M.A., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.*

PART III.—*Applications of the Dynamical Theory to establish Relations between the Physical Properties of all Substances.*

44. THE two fundamental equations of the dynamical theory of heat, investigated above, express relations between quantities of heat required to produce changes of volume and temperature in any material medium whatever, subjected to a uniform pressure in all directions, which lead to various remarkable conclu-

sions. Such of these as are independent of Joule's principle (expressed by equation (2) of § 20), being also independent of the truth or falseness of Carnot's contrary assumption regarding the permanence of heat, are common to his theory and to the dynamical theory; and some of the most important of them[*] have been given by Carnot himself, and other writers who adopted his principles and mode of reasoning without modification. Other remarkable conclusions on the same subject might have been drawn from the equation $\frac{dM}{dt} - \frac{dN}{dv} = 0$, expressing Carnot's assumption (of the truth of which experimental tests might have been thus suggested); but I am not aware that any conclusion deducible from it, not included in Carnot's expression for the motive power of heat through finite ranges of temperature, has yet been actually obtained and published.

45. The recent writings of Rankine and Clausius contain some of the consequences of the fundamental principle of the dynamical theory (expressed in the first fundamental proposition above) regarding physical properties of various substances; among which may be mentioned especially a very remarkable discovery regarding the specific heat of saturated steam (investigated also in this paper in § 58 below), made independently by the two authors, and a property of water at its freezing-point, deduced from the corresponding investigation regarding ice and water under pressure by Clausius; according to which he finds that, for each $\frac{1}{10}$° Cent. that the solidifying point of water is lowered by pressure, its latent heat, which under atmospheric pressure is 79, is diminished by ·081. The investigations of both these writers involve fundamentally various hypotheses which may be or may not be found by experiment to be approximately true; and which render it difficult to gather from their writings what part of their conclusions, especially with reference to air and gases, depend merely on the necessary principles of the dynamical theory.

46. In the remainder of this paper, the two fundamental propositions, expressed by the equations

$$\frac{dM}{dt} - \frac{dN}{dv} = \frac{1}{J}\frac{dp}{dt} \quad \cdot \quad \cdot \quad \cdot \quad (2) \text{ of § 20,}$$

and

$$M = \frac{1}{\mu} \cdot \frac{dp}{dt}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3) \text{ of § 21,}$$

are applied to establish properties of the specific heats of any substance whatever; and then special conclusions are deduced for the case of a fluid following strictly the "gaseous laws" of

[*] See above, § 22.

density, and for the case of a medium consisting of parts in different states at the same temperature, as water and saturated steam, or ice and water.

47. In the first place it may be remarked, that by the definition of M and N in § 20, N must be what is commonly called the "specific heat at constant volume" of the substance, provided the quantity of the medium be the standard quantity adopted for specific heats, which, in all that follows, I shall take as the unit of weight. Hence the fundamental equation of the dynamical theory, (2) of § 20, expresses a relation between this specific heat and the quantities for the particular substance denoted by M and p. If we eliminate M from this equation, by means of equation (3) of § 21, derived from the expression of the second fundamental principle of the theory of the motive power of heat, we find

$$\frac{d\mathrm{N}}{dv} = \frac{d\left(\frac{1}{\mu}\frac{dp}{dt}\right)}{dt} - \frac{1}{\mathrm{J}}\frac{dp}{dt} \quad . \quad . \quad . \quad (14),$$

which expresses a relation between the variation in the specific heat at constant volume, of any substance, produced by an alteration of its volume at a constant temperature, and the variation of its pressure with its temperature when the volume is constant; involving a function, μ, of the temperature, which is the same for all substances.

48. Again, let K denote the specific heat of the substance under constant pressure. Then, if dv and dt be so related that the pressure of the medium, when its volume and temperature are $v + dv$ and $t + dt$ respectively, is the same as when they are v and t, that is, if

$$0 = \frac{dp}{dv}\,dv + \frac{dp}{dt}\,dt\,;$$

we have

$$\mathrm{K}dt = \mathrm{M}dv + \mathrm{N}dt.$$

Hence we find

$$\mathrm{M} = \frac{-\dfrac{dp}{dv}}{\dfrac{dp}{dt}}(\mathrm{K} - \mathrm{N}) \quad . \quad . \quad . \quad . \quad (15),$$

which merely shows the meaning in terms of the two specific heats, of what I have denoted by M. Using in this for M its value given by (3) of § 21, we find

$$\mathrm{K} - \mathrm{N} = \frac{\left(\dfrac{dp}{dt}\right)^2}{\mu \times -\dfrac{dp}{dv}} \quad . \quad . \quad . \quad . \quad (16),$$

an expression for the difference between the two specific heats, derived without hypothesis from the second fundamental principle of the theory of the motive power of heat.

49. These results may be put into forms more convenient for use, in applications to liquid and solid media, by introducing the notation :—

$$\left.\begin{array}{c} \kappa = v \times -\dfrac{dp}{dv} \\[2mm] e = \dfrac{1}{\kappa}\dfrac{dp}{dt} \end{array}\right\} \quad \ldots \ldots \quad (17),$$

where κ will be the reciprocal of the compressibility, and e the coefficient of expansion with heat.

Equations (14), (16) and (3), thus become

$$\frac{d\mathrm{N}}{dv} = \frac{d\left(\dfrac{\kappa e}{\mu}\right)}{dt} - \frac{\kappa e}{\mathrm{J}} \quad \ldots \ldots \quad (18),$$

$$\mathrm{K} - \mathrm{N} = v\frac{\kappa e^2}{\mu} \quad \ldots \ldots \quad (19),$$

$$\mathrm{M} = \frac{1}{\mu}.\kappa e \quad \ldots \ldots \quad (20);$$

the third of these equations being annexed to show explicitly the quantity of heat developed by the compression of the substance kept at a constant temperature. Lastly, if θ denote the rise in temperature produced by a compression from $v + dv$ to v before any heat is emitted, we have

$$\theta = \frac{1}{\mathrm{N}} \cdot \frac{\kappa e}{\mu}. dv = \frac{\kappa e}{\mu \mathrm{K} - v\kappa e^2} dv \quad \ldots \quad (21).$$

50. The first of these expressions for θ shows that, when the substance contracts as its temperature rises (as is the case, for instance, with water between its freezing-point and its point of maximum density), its temperature would become lowered by a sudden compression. The second, which shows in terms of its compressibility and expansibility exactly how much the temperature of any substance is altered by an infinitely small alteration of its volume, leads to the approximate expression

$$\theta = \frac{\kappa e}{\mu \mathrm{K}},$$

if, as is probably the case, for all known solids and liquids, e be so small that $e \cdot v\kappa e$ is very small compared with $\mu \mathrm{K}$.

51. If, now, we suppose the substance to be a gas, and introduce

the hypothesis that its density is strictly subject to the "gaseous laws," we should have, by Boyle and Mariotte's law of compression,

$$\frac{dp}{dv} = -\frac{p}{v} \quad \cdots \quad \cdots \quad (22);$$

and by Dalton and Gay-Lussac's law of expansion,

$$\frac{dv}{dt} = \frac{\mathrm{E}v}{1+\mathrm{E}t} \quad \cdots \quad \cdots \quad (23);$$

from which we deduce

$$\frac{dp}{dt} = \frac{\mathrm{E}p}{1+\mathrm{E}t}.$$

Equation (14) will consequently become

$$\frac{d\mathrm{N}}{dv} = \frac{d\left\{ \dfrac{\mathrm{E}p}{\mu(1+\mathrm{E}t)} - \dfrac{p}{\mathrm{J}} \right\}}{dt} \quad \cdots \quad (24),$$

a result peculiar to the dynamical theory and equation (16),

$$\mathrm{K}-\mathrm{N} = \frac{\mathrm{E}^2 pv}{\mu(1-\mathrm{E}t)^2} \quad \cdots \quad \cdots \quad (25),$$

which agrees with the result of § 53 of my former paper.

If V be taken to denote the volume of the gas at the temperature $0°$ under unity of pressure, (25) becomes

$$\mathrm{K}-\mathrm{N} = \frac{\mathrm{E}^2 \mathrm{V}}{\mu(1+\mathrm{E}t)} \quad \cdots \quad \cdots \quad (26).$$

52. All the conclusions obtained by Clausius, with reference to air or gases, are obtained immediately from these equations by taking

$$\mu = \mathrm{J}\,\frac{\mathrm{E}}{1+\mathrm{E}t},$$

which will make $\dfrac{d\mathrm{N}}{dv} = 0$, and by assuming, as he does, that N, thus found to be independent of the density of the gas, is also independent of its temperature.

53. As a last application of the two fundamental equations of the theory, let the medium with reference to which M and N are defined consist of a weight $1-x$ of a certain substance in one state, and a weight x in another state at the same temperature, containing more latent heat. To avoid circumlocution and to fix the ideas, in what follows we may suppose the former state to be liquid and the latter gaseous; but the investigation, as will be seen, is equally applicable to the case of a solid in contact with the same substance in the liquid or gaseous form.

54. The volume and temperature of the whole medium being,

as before, denoted respectively by v and t, we shall have

$$\lambda(1-x) + \gamma x = v \quad . \quad . \quad . \quad . \quad (27),$$

if λ and γ be the volumes of unity of weight of the substance in the liquid and the gaseous states respectively : and p, the pressure, may be considered as a function of t, depending solely on the nature of the substance. To express M and N for this mixed medium, let L denote the latent heat of a unit of weight of the vapour, c the specific heat of the liquid, and h the specific heat of the vapour when kept in a state of saturation. We shall have

$$M dv = L \frac{dx}{dv} dv$$

$$N dt = c(1-x)dt + hx dt + L \frac{dx}{dt} dt.$$

Now, by (27), we have

$$(\gamma - \lambda)\frac{dx}{dv} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (28),$$

and

$$(\gamma - \lambda)\frac{dx}{dt} + (1-x)\frac{d\lambda}{dt} + x\frac{d\gamma}{dt} = 0 \quad . \quad . \quad (29).$$

Hence

$$M = \frac{L}{\gamma - \lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30),$$

$$N = c(1-x) + hx - L\frac{(1-x)\frac{d\lambda}{dt} + x\frac{d\gamma}{dt}}{\gamma - \lambda} \quad (31).$$

55. The expression of the second fundamental proposition in this case becomes, consequently,

$$\mu = \frac{(\gamma - \lambda)\frac{dp}{dt}}{L} \quad . \quad . \quad . \quad . \quad . \quad . \quad (32),$$

which agrees with Carnot's original result, and is the formula that has been used (referred to above in § 31) for determining μ by means of Regnault's observations on steam.

56. To express the conclusion derivable from the first fundamental proposition, we have, by differentiating the preceding expressions for M and N with reference to t and v respectively,

$$\frac{dM}{dt} = \frac{1}{\gamma - \lambda} \cdot \frac{dL}{dt} - \frac{L}{(\gamma - \lambda)^2} \cdot \frac{d(\gamma - \lambda)}{dt}$$

$$\frac{dN}{dv} = \left(h - c - L\frac{\frac{d\gamma}{dt} - \frac{d\lambda}{dt}}{\gamma - \lambda} \right)\frac{dx}{dv}$$

$$= \left\{ \frac{h - c}{\gamma - \lambda} - \frac{L}{(\gamma - \lambda)^2} \right\}\frac{d(\gamma - \lambda)}{dt}.$$

Hence equation (2) of § 20 becomes

$$\frac{\frac{d\mathrm{L}}{dt}+c-h}{\gamma-\lambda}=\frac{1}{\mathrm{J}}\frac{dp}{dt} \quad . \quad . \quad . \quad . \quad (33).$$

Combining this with the conclusion (32) derived from the second fundamental proposition, we obtain

$$\frac{d\mathrm{L}}{dt}+c-h=\frac{\mathrm{L}\mu}{\mathrm{J}} \quad . \quad . \quad . \quad . \quad (34).$$

The former of these equations agrees precisely with one which was first given by Clausius, and the preceding investigation is substantially the same as the investigation by which he arrived at it. The second differs from another given by Clausius only in not implying any hypothesis as to the form of Carnot's function μ.

57. If we suppose μ and L to be known for any temperature, equation (32) enables us to determine the value of $\frac{dp}{dt}$ for that temperature; and thence deducing a value of dt, we have

$$dt=\frac{\gamma-\lambda}{\mu\mathrm{L}}dp \quad . \quad . \quad . \quad . \quad . \quad (35);$$

which shows the effect of pressure in altering the "boiling-point" if the mixed medium be a liquid and its vapour, or the melting-point if it be a solid in contact with the same substance in the liquid state. This agrees with the conclusion arrived at by my elder brother in his Theoretical Investigation of the Effect of Pressure in Lowering the Freezing-Point of Water*. His result, obtained by taking as the value for μ that derived from Table I. of my former paper for the temperature $0°$, is that the freezing-point is lowered by $·0075°$ Cent. by an additional atmosphere of pressure. Clausius, with the other data the same, obtains $·00733°$ as the lowering of temperature by the same additional pressure, which differs from my brother's result only from having been calculated from a formula which implies the hypothetical expression $\mathrm{J}\dfrac{\mathrm{E}}{1+\mathrm{E}t}$ for μ. It was by applying equation (33) to determine $\frac{d\mathrm{L}}{dt}$ for the same case that Clausius arrived at the curious result regarding the latent heat of water under pressure mentioned above (§ 45).

58. Lastly, it may be remarked that every quantity which ap-

* Transactions, vol. xvi. part 5. His paper was republished, with some slight modifications, in the Cambridge and Dublin Mathematical Journal, new series, vol. v.—Nov. 1850.

pears in equation (33), except h, is known with tolerable accuracy for saturated steam through a wide range of temperature; and we may therefore use this equation to find h, which has never yet been made an object of experimental research. Thus we have

$$-h = \frac{\gamma - \lambda}{J} \frac{dp}{dt} - \left(\frac{dL}{dt} + c\right).$$

For the value of γ the best data regarding the density of saturated steam that can be had must be taken. If for different temperatures we use the same values for the density of saturated steam (calculated according to the gaseous laws, and Regnault's observed pressure from $\frac{1}{1693 \cdot 5}$, taken as the density at $100°$), the values obtained for the first term of the second member of the preceding equation are the same as if we take the form

$$-h = \frac{L\mu}{J} - \left(\frac{dL}{dt} + c\right)$$

derived from (34), and use the values of μ shown in Table I. of my former paper. The values of $-h$ in the second column in the following table have been so calculated, with, besides, the following data afforded by Regnault from his observations on the total heat of steam, and the specific heat of water

$$\frac{dL}{dt} + c = \cdot 305.$$

$$L = 606 \cdot 5 + \cdot 305 t - (\cdot 00002 t^2 + \cdot 0000003 t^3).$$

The values of $-h$ shown in the third column are those derived by Clausius from an equation which is the same as what (34) would become if $J \dfrac{E}{1 + Et}$ were substituted for μ.

t.	$-h$ according to Table I. of "Account of Carnot's Theory."	$-h$ according to Clausius.
0	1·863	1·916
50	1·479	1·465
100	1·174	1·133
150	0·951	0·879
200	0·780	0·676

59. From these results it appears, that through the whole range of temperatures at which observations have been made, the value of h is negative; and, therefore, if a quantity of saturated vapour be compressed in a vessel containing no liquid water, heat must be continuously abstracted from it in order

that it may remain saturated as its temperature rises; and conversely, if a quantity of saturated vapour be allowed to expand in a closed vessel, heat must be supplied to it to prevent any part of it from becoming condensed into the liquid form as the temperature of the whole sinks. This very remarkable conclusion was first announced by Mr. Rankine, in his paper communicated to this Society on the 4th of February last year. It was discovered independently by Clausius, and published in his paper in Poggendorff's *Annalen* in the months of April and May of the same year.

60. It might appear at first sight, that the well-known fact that steam rushing from a high-pressure boiler through a small orifice into the open air does not scald a hand exposed to it[*], is inconsistent with the proposition, that steam expanding from a state of saturation must have heat given to it to prevent any part from becoming condensed; since the steam would scald the hand unless it were dry, and consequently above the boiling-point in temperature. The explanation of this apparent difficulty, given in a letter which I wrote to Mr. Joule last October, and which has since been published in the Philosophical Magazine, is, that the steam in rushing through the orifice produces mechanical effect which is immediately wasted in fluid friction, and consequently reconverted into heat; so that the issuing steam at the atmospheric pressure would have to part with as much heat to convert it into water at the temperature $100°$ as it would have had to part with to have been condensed at the high pressure and then cooled down to $100°$, which for a pound of steam initially saturated at the temperature t is, by Regnault's modification of Watt's law, $\cdot305(t-100°)$ more heat than a pound of saturated steam at $100°$ would have to part with to be reduced to the same state; and the issuing steam must therefore be above $100°$ in temperature, and dry.

[* *Note added June 26*, 1852.—At present I am inclined to believe that the rapidity of the current exercises a great influence on the sensation experienced in the circumstances, by causing the steam to mix with the surrounding air; for I have found that the hand suffers pain when exposed to the steam issuing from a common kettle, and dried by passing through a copper tube surrounded by red-hot coals or heated by lamps. But although there may be uncertainty regarding the causes of the different sensations in the different circumstances, I believe there is no reason for doubting either the fact of the dryness of the steam issuing from a high-pressure boiler (except when there is "priming" to a considerable extent), or the correctness of the explanation of this fact which I have given in the letter referred to.]

XXVI. *Renewed Inquiries concerning the Spiral Structure of Muscle, with Observations on the Muscularity of Cilia.* By MARTIN BARRY, *M.D., F.R.S.*

[Concluded from p. 98.]

On the Muscularity of Cilia.

AS his previous observations had led him to expect, cilia were found to be no other than his twin or double spirals. No man, he thinks, will do him the injustice to suppose he maintains the possibility of discerning a double spiral in the minutest cilium. He is as far from maintaining this as he is from asserting the possibility of seeing a double spiral in the minutest muscular fibril. But he does maintain that those who undertake the examination of cilia in the way in which they should set about the examination of all organic tissues, *i. e.* with a desire to know how they originate, what is the history of their development, will certainly find that the double spiral is the fundamental form of all cilia the structure of which can be reached with the microscope, and therefore probably of the most minute. Indeed under favourable circumstances, traces thereof are not so very rarely to be discerned, by the accustomed eye, even in the latter.

In the author's observations he used several bivalve mollusca, including the Oyster, *Ostrea edulis,* and the common Sea Mussel, *Mytilus edulis.* The one last mentioned is to be preferred, because of the bars of its branchial laminæ being most easy of separation. And this mussel is further recommended to those disposed to repeat the author's observations, on account of the excellent description of its gills given by Dr. Sharpey in his Article "Cilia" in Todd's Cyclopædia of Anatomy and Physiology. He recommends the examination of this Mussel when small, because of the branchial laminæ being more transparent than in the larger specimens. He examined some in which the shell measured scarcely two lines in length, they being the smallest he could obtain. The most convenient size, however, he found to be that in which the shell measured in length from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch. Still an examination of the largest should not be omitted.

Convinced by his earlier microscopic labours that it is best to direct the eye for a considerable time exclusively to the same part or set of objects in order to enable it to detect minute differences in the same or in different individuals, the author directed his solely to the branchial laminæ, and here to little more than the sides of those parallel bars of which the branchial laminæ are composed. In this way it was that he became acquainted with the fact, that, as the ever-acting heart requires a continued renewal of its fibrils, so are new generations continually preparing to succeed the indefatigably vibrating cilia.

Before detailing his observations, the author states what others should do who may be disposed to repeat the examinations. A small piece, about a square line, having been cut from the marginal edge of the gill and placed upon glass, he adds to it a drop of the fluid, which, on the mussel being opened, collects in the shell, gently and to a small extent separates the bars from one another with fine needles, and places them under the microscope without the addition of any covering such as glass or mica. It is soon seen that some of the bars, wedge-like in their transverse sections, present the thicker of their edges to the eye, fig. 37, while others are lying on their sides, figs. 41, 36. Both should be examined with especial reference to the cilia on the two sides of the bar. Of these cilia there are three sets, and not two only, as hitherto supposed; one set uppermost when the thicker edge of the bar is directed towards the eye, and marked *m* in the figures just referred to; the second occupying a middle place, and marked *n* in the same figures; the third lowest, and marked *o*.

Concerning these cilia, the author states the following as new facts:—In the *first* place, these cilia, and from analogy probably all cilia, consist of double spiral threads, and thus have a structure like that of the muscular fibril; *secondly,* the cilia *m,* fig. 36, &c., present merely stages in the development of the cilia *n*; *thirdly,* the cilia *o,* in the same figures, hitherto either overlooked or held to be identical with the cilia *n,* are really not so,—they are the counterpart thereof. And he then proceeds to establish these three positions in the order here laid down, as follows:—

Separated from their localities by manipulation, and strewn through the field of view among the fragments of the gill, are seen simple cells, several of which are represented in outline in fig. 27. In the interior of such cells are the young cilia indicated. They push before them the membrane of the cell, so that it appears pointed; and afterwards present themselves, as in fig. 28, of a club-like form*. Sometimes this club-like form appears referrible to a provision of plastic substance at the extremity for the lengthening of the cilium, and sometimes to a bending down of the extremity hook-like upon itself. Up to this time the membrane of the cell appears in some instances to continue entire,—the young cilium, though coming into view, being as it were still unborn. At length the membrane is ruptured, and the bent down extremity of the cilium gradually developes and unrolls itself like a young fern, fig. 29. This figure, fig. 29, represents part of a large fragment, several of which were found in substance scraped from the gill of the Oyster.

* Probably Valentin saw the same stage of development in *Unio pictorum,* where he mentions it (the " club-like form ") as an unusual shape (" ausnahmsweiser Gestalt ").—R. Wagner's *Handwörterbuch der Physiologie,* p. 500.

They contained at the margin numerous cells. The middle space presented none. On the nature of these large fragments the author has for the present nothing further to remark, than that they afforded him an invaluable contribution towards the history of development of cilia; for of that development they presented with distinctness a very early stage. The minute cells in their interior seemed destined to give origin to cilia, which here and there, fig. 29, were seen to have been already formed and to have burst through the membrane of their cells. One of these, the interior of which was seen with rare distinctness, is represented on a larger scale in fig. 30. The young cilium here drawn consisted of two spirals, within the winds of which was a pellucid substance corresponding to that which the author above and elsewhere has termed hyaline. At the extremity the two spirals passed into one another, and were bent over hook-like towards one side. At the base they separated, to bestride, as it were, the contents of the cell in which the cilium had been formed*. Perhaps these two separate threads may be considered as the radical ends of the cilium, in which growth first of all takes place somewhat in the following manner:—The extremity of each of these two threads draws into itself new substance from the nucleus of the cell. And now as the cilium is alternately in the states of twisting and untwisting, it gradually spins up into its substance those after-threads, and in this manner elongates.

Drawings are then given of stages following those just described, of which figs. 31, 32, 33, and 34 present a selection. These different appearances evidently denote different degrees of development. [Corresponding differences were noticed in their movements. None of them, however, were in a perfect state. For the movements of even the most advanced were awkward, showing them to be, as it were, still in their apprenticeship.] Now to all of these cilia, making allowance for differences in the degree of development, may be applied the description just given of fig. 30; though it is only here and there that a trace of connexion with the cell, such as that in fig. 30, can be distinctly seen. The author thinks that no observer can attentively examine such cilia without seeing, as he did, that each of them consists of a double spiral thread, having therefore a structure like that of the muscular fibril, and thus establishing his position No. 1.

The broad cilia of which examples have been given in Beroë and other ciliograde Mollusca,—where, instead of cilia of usual form and arrangement, there are found rows of broad flat flaps each of which is said to consist of a row of single cilia,—appear to

* [It will be observed, that each of these separate threads is twisted on itself.]

the author to consist of *fasciculi* of cilia; and if this be the case, their mode of reproduction is probably the same as that of other muscle. He found the bulb at the base of some cilia much smaller than at that of others. This may have arisen from division,—a larger bulb together with its cilium dividing into two; or it may have arisen from consumption of the bulb, through nourishment and growth of the cilium. In other cases the bulb had entirely disappeared, and the cilia arose from a common ground, fig. 35 *h*. Here it is possible that after the bulbs had been entirely spent from the growth of the cilia, all trace thereof had disappeared.

Notwithstanding all that he has said, both in this treatise and in former ones, on the necessity in all researches on the structure of tissues of attending to the history of their development, the author adds that he feels called upon candidly to acknowledge cilia to present in this respect a difficulty such as perhaps is scarcely to be found elsewhere. Here the observer has it not in his power to *begin* with the history of development; for after what has been above stated of a continued renewal of the cilia, and of several stages in their development being sometimes met with even in the same bar, it cannot be expected that the younger cilia will necessarily be found in the younger mussels. The few facts in their development recorded in this memoir, were not fully ascertained until after a long series of measurements and observations on movements, and on forms of cilia met with *quite at random*. The author trusts his descriptions and drawings of the several stages may be useful to others in following out the history of development; b t it is a misfortune for him not to have it in his power to say just where the younger and most convincing stages are to be found, such as would enable others so easily to confirm his observations on their spiral structure. It is added: "You may open a very large number of mussels, and devote whole days to the examination, before you find an example for demonstration. If, however, you are so fortunate as to meet with a stage such as that in fig. 30, you feel richly rewarded for all the labour."

The author then proceeds to establish his position No. 2, that the cilia marked *m* in fig. 36, &c. are merely different stages in development of the cilia *n* in the same figures. To this unexpected observation he was led by the following facts.

In the *first* place, the cilia *m* and *n*, see fig. 36, have a common place of origin, their roots arising mixed together in the same field. *Secondly*. You here and there see one of the cilia *m* flexed at its base, by which its extremity is made to approach the ex-

tremities of the cilia n; but it instantly returns to its previous state, to be immediately afterwards depressed again as before; and so on*.   *Thirdly.* You sometimes meet with states in which this depression of some of the cilia m is permanent. See m' in the same figure.   *Fourthly.* As already mentioned, the cilia m in different individuals present very different states, figs. 31, 32, 33, 34, 39, 40.   Their lengths differ, some being very short; sometimes they are straight, sometimes curved; sometimes they are found moving, sometimes motionless.   The movements are generally quite irregular.   There is nothing like a common purpose in them; certainly no combination for the production of a current.   In some they are such as to suggest the idea of efforts to become unbent at their extremities, fig. 32; and in others no longer bent at their extremities, the movements seem made for the purpose of becoming elongated, fig. 40.   You sometimes meet with the two last-mentioned states in the same bar, fig. 39. In short, these different appearances and movements evidently denote different degrees of development.   Not until they reach the state in fig. 36 can the cilia m be said to have attained maturity, and to exhibit a common purpose in their movements. But even then their movements are not so vehement as to be likely soon to wear them out.   Why, then, are they so constantly renewed?   The fact is, that by flexion at the base, the cilia m (see fig. 36) pass, one after another, into the vehemently vibrating cilia n, which they succeed as a later generation.   For this purpose they are formed, and then for the first time do they perform really efficient action.   Thus it was that the author was led to his position No. 2.

In his third conclusion, he stated that the cilia o in fig. 36, &c., hitherto either overlooked or held to be identical with the cilia n, are really not so,—they are the counterpart thereof. This will be immediately made clear if attention be paid to their origin and the function they perform.

The roots of these two lines of cilia are separated by a broad pellucid space, fig. 36 h, in which are no cells such as those (p) giving origin to the cilia in question.   The cilia of the two lines, proceeding from opposite sides of the pellucid space, arch over it, their extremities meeting in the middle line, where they, alternating with one another, like the fingers of the two hands, form

* The movements of the cilia m are described by Sharpey merely as follows:—"The more opake cilia, or those of the exterior range, appear and disappear by turns, as if they were continually changing from a horizontal to a vertical direction and back again."—*L. c.* p. 622.   And the author says, he is not aware that any other author has given more exact information concerning them.

a sort of tunnel, through which water is driven by their vehement movements.   So much for conclusion No. 3*.

On the subject of functions it is also to be remarked, that the pellucid space, fig. 36 *h*, over which these two lines of cilia, *n* and *o*, move so vehemently, belongs to the membrane of the bar (known to be considered as a vessel of the gill), which membrane is probably destined to absorb oxygen from the water and communicate it to the blood.   This would be materially assisted were the stream of water accelerated, and a fresh supply of oxygen constantly afforded.

It must further be remembered, that, as is known, the direction of the current in neighbouring bars is different.   If in one it is from the base of the gill towards its margin, in the next it is from the margin towards the base; in the one case appearing to end at a round projection covered with vibrating cilia, fig. 41 *qq*,—in the other appearing to begin there.   The direction of the currents now mentioned as opposite in neighbouring bars, is also opposite on the two sides of the same bar, figs. 37, 38†.   The round projection, fig. 41 *qq*, just referred to, Sharpey has not particularly mentioned.   It seems to be of the same nature as his "round projections," *q* in the same figure, with this difference, that where the two bars pass into one another at their ends, two round projections pass into one.   Hence the larger size of that at *qq*, fig. 41.

The marvellously complicated movements of the cilia *n* and *o*, figs. 36, 37, 38, 41, the author says he has very often observed, continuing to watch them until they became slower, and at length ceased.   At last only groups of them are seen thus moving, then not more than two or three together, and finally single ones.

* The cilia *o*, as an independent line, Sharpey appears not to have observed; he mentions and figures merely the cilia *n*, as is evident from the following :—" The motion of the other set consists in a succession of undulations, which proceed in a uniform manner along the sides of the bar from one end to the other.   It might be very easily mistaken for the circulation of globules of a fluid within a canal, more especially as the course of the undulations is different on the two sides of the bar, being directed on one side towards the edge of the gill, and on the other towards the base. But besides that the undulations continue for some time in small pieces cut off from the gill, which is inconsistent with the progression of fluid in a canal, the cilia are easily distinguished when the undulatory motion becomes languid.   When it has entirely ceased, they remain in contact with each other, so as to present the appearance of a membrane (*d*, *d*, fig. F)."— Sharpey, *l. c.* p. 623.

So far Dr. Sharpey.   And the author adds, that he is not aware of any other observer having made any mention of them,—the cilia *o*.

† [This fact also is already known.]

When the movements have entirely ceased, the two lines of cilia lie nearly parallel, fig. 36, *n, o,* and somewhat bent, with the convexity almost always in the same direction as the current their movements had occasioned.

It remains to be added respecting the cilia *n* and *o,* that when their movements have terminated, and the cilia are left in a state of relaxation, they often in a short time entirely disappear. Probably most of them break off at their roots, as indeed may constantly happen during life, when the old ones become replaced by new, the former going off when worn out, being carried away by the stream, and thrown out at the excretory orifice.

As nothing until now was known regarding the structure of cilia, everything brought forward as to the cause of their movements has been conjecture only. Having found in them a structure adapted for contraction and relaxation, the author has much pleasure in thus showing that his fellow-countryman, Professor Sharpey, was right when in the year 1836 he thought it probable that the moving power of cilia lay in the cilia themselves, and was referrible to a substance contained in more or less of their length, like that of muscle.

The undulatory movements of cilia,—compared by Sharpey, when many were seen together, to those produced by the wind on a field of corn,—the author on two occasions witnessed when performed by cilia in a single line, and when most perfect; on one of which occasions he had the pleasure of showing the rare spectacle to Purkinje. In both instances this living mechanism was seen at the marginal end of the bar, and in the line of cilia *m,* fig. 41; in one instance at the point *r,* in the other at the point marked *s.* The rough diagram, fig. 42, will scarcely serve to convey an idea of these undulatory movements, for the appearance was exceedingly delicate and beautiful. The undulating cilia in the two instances were in different conditions. In the one instance they had their spirals in a twine-like state, as in fig. 40, and were permanently contracted at no part; in the other instance they were permanently contracted at the base, as at *m* in fig. 36. In the first case the movements may have consisted merely of a shortening and lengthening in the axis of the cilia; in the second, of flexion at the base. Further, the cilia in the two being instances in question were of different forms; in one instance being straight, as in fig. 40,—in the other curved, as in fig. 36 *m.* As now the contraction of a double spiral implies a twisting of the same, the extremity when bent must describe a course spirally infundibular, not represented in the diagram. [The author observed very young cilia, fig. 43, which evidently

showed in their movements a shortening and lengthening. No definite order, however, such as that implied by undulation, was observed. Perhaps a disturbance had occurred through manipulation.]

It is important, the author thinks, to have seen these undulatory movements performed by the cilia *m*, fig. 36; for, as successors to the cilia *n*, the cilia thus undulating were about to arrange themselves in one of the two lines above mentioned as combining to form a sort of tunnel, through which by their extremely vehement movements to drive a rapid current. And the following occurred to him as possibly sufficient to explain the appearance presented by these movements,—which have been aptly compared to the rapid flow of globules of a fluid. The cilia *n*, fig. 36, are all bent in the same direction; they are arranged in a line, and perform their swinging or lashing movements in an undulatory manner according to the order of their positions in that line. Like movements, and in the like order, are performed by the cilia *o* in the opposite line; their extremities alternating with the extremities of the cilia *n* in the first line, like the fingers of the two hands, and moving without the slightest mutual interference. Now were the movements throughout the whole phalanx of cilia contemporaneous, there would be presented to the eye a permanent line of swinging movements. As, however, those swinging movements are performed by the cilia one after another in the order of their positions in the line, they assume the appearance of a row of roundish waves, following, or as it were chasing, and uninterruptedly passing into one another; not rarely appearing to the eye like a long revolving screw. The difference between rows of globules (the appearance most frequently presented by the movements in question) and screw-cylinders, may be supposed to arise as follows:—When the swinging movements are of different extent at different parts, we have the appearance comparable to a row of globules; when those movements pass uniformly into one another, there is seen the long-revolving screw*.

Having found the cilia on the branchial laminæ of Mussels to consist of double spirals, the author deems it scarcely needful to remark, that he infers a like structure in other cilia, exist where they may. As, however, in the course of these researches he has very often had the opportunity of examining cilia of Infusoria, several species of which are met with in the fluid of the Mussel's shell, he cannot refrain from making known the fact, that in these cilia also he finds his double spiral. Often did he

* [The screw probably exhibiting the normal, and the row of globules a disturbed state.]

see in them the spiral structure with such distinctness, as to feel astonished at its not having been long since observed. As the tails of spermatozoa of course correspond to cilia, their structure must be essentially the same. He states it to be now nine years since he published his observation of the spiral structure of the tail of the mammiferous spermatozoon (Phil. Trans. 1842, p. 107). It is probably owing to a like refractive power in the spirals and in the hyaline which lies between them, that the spirals are so difficult to distinguish in the tails of spermatozoa; and hence it no doubt is that they were not observed before.

The subject of the present paper being the structure of muscle, the author has avoided the special mention of other tissues. Lest, however, from this omission it should be supposed that he has abandoned his views,—that the structure of all the elementary fibres, as well of plants as of animals, is originally spiral,—he thinks it right before concluding briefly to declare that those views remain unchanged. Bowman says: "Dr. Barry might as well have entitled his paper 'On the Spiral Structure of the Organic World*.'" To this title, satirically proposed by Bowman, the author remarks that he has not the least objection; so far, indeed, is he from being thereby annoyed, that he thanks him for it. He thanks Prof. Bowman for having thus recorded in the Cyclopædia of Anatomy and Physiology, as far back as in the year 1842, that his (Dr. Barry's) views in regard to the spiral structure of organic fibre were universal in their character; "and I am convinced," it is added, "that the day will come when my views will be as universally adopted by physiologists, as I myself am convinced that the spiral structure is universal. Let it only be fully understood what those views are. What I maintain is, that the spiral form of fibre everywhere is the original and incipient form; and that if this form be lost in many tissues in the course of their special development, it remains permanent in the fibre of muscle as a necessary attribute of its function."

In a postscript it is added, that in the contractile stem of the Bell polype (*Vorticella convallaria*), of which several specimens were examined, the author found his double spiral. In relaxation, this double spiral lay in its extended cylindrical gelatinous sheath, (which he regards as its elastic sarcolemma) in [elongated] spiral winds. In contraction, it presented itself in a manner about the same as that in fig. 18; with this difference, that the double spiral in the polype was enclosed in its gelatinous sarcolemma, which that figure, representing quite another object, does not show.

* Cyclopædia of Anat. and Phys., art. "Muscle and Muscular Motion," p. 511.

XXVII. *On the Chemical Constitution and Atomic Weight of the new Polarizing Crystals produced from Quinine.* By WILLIAM BIRD HERAPATH, *M.D.**

IN the March Number of this Journal the author announced the discovery of a peculiar salt of quinine, which possessed the power of polarizing a ray of light with even greater intensity than the tourmaline; and at certain angles of rotation it also depolarized light, and acted as selenite would do under similar circumstances.

He then stated that the qualitative analysis showed this salt to be a compound of quinine, iodine, and sulphuric acid; and although the relative quantities of these constituents had not at that time been estimated, he gave it the name of iodide of disulphate of quinine. In the present communication, the results of the quantitative chemical analysis of this compound will be detailed; and it will be evident that a new idea of its constitution will be elicited, which will render another name necessary, and more in accordance with the results specified.

Before attempting the analysis, it was of course necessary to invent a process which would furnish a large quantity of this substance at one operation; after several attempts, with more or less success, the following method was adopted, which at the same time served as a means of corroborating the results of the future analysis, as it enabled the experimenter to account for all the iodine used in the operation.

A tubulated retort was adapted to a receiver by careful connections, and the latter adjusted to a second receiver, somewhat in the manner of a Wolff's apparatus; the condensers were then surrounded by a freezing mixture of nitre and hydrochlorate of ammonia. Into the retort were placed 100 grs. of pure disulphate of quinine, 3 fluid ounces of pyroligneous acid, 2 drms. of diluted sulphuric acid (containing about 12 grs. of dry acid); when this mixture had been raised to about 180° Fahr., the alcoholic solution of iodine was gradually added through a bent glass funnel adapted to the tubule of the retort. In this manner 30 grs. of iodine dissolved in 1150 grs. of alcohol were employed; the whole operation occupied about half an hour, during which period a reddish-coloured fluid was collected in the receivers, about 4 fluid drachms in quantity; this of course was carefully set aside for examination.

The whole was allowed to grow cold, still in connection; an abundant crop of crystals formed in the retort, which, having been kept during twenty-four hours at a temperature of 40° Fahr. to deposit, were collected on a filter, and washed several

* Communicated by the Author.

times with acetic acid at 40° Fahr., which had been previously found to have little solvent power on this compound at that temperature. The crystals having been well washed, were dissolved in boiling alcohol, spec. grav. ·838, and on cooling they recrystallized; this operation having been repeated, they were at length obtained pure from any admixture of disulphate of quinine. Having been drained on a filter and washed with cold spirit, they were dried at 90° Fahr., then over sulphuric acid, and weighed: 66·6 grs. were obtained by this operation.

The acid mother-liquid, together with the first washings, were then examined for iodine; upon allowing a few drops to evaporate spontaneously on a slip of glass, polarizing crystals formed around the edge of the liquid; consequently the compound is slightly soluble in cold acid. The acetic acid having been nearly neutralized by ammonia, nitrate of silver was dropped into the solution as long as any iodide of silver was deposited; this was then carefully collected on a filter, washed repeatedly with distilled water, then with ammonia to remove any chloride, again with distilled water, dried and ignited; it weighed 2·00 grs. = iodine 1·08.

The alcoholic mother-liquids and washings were then examined for iodine, and crystals were similarly obtained upon spontaneous evaporation. In order to precipitate the iodine, a silver salt was used, and dropped into the solution as long as any cloudiness was produced; the whole thrown on a filter, and the precipitated iodide of silver, washed with diluted nitric acid to remove any quinine, and subsequently with ammonia to take up any chloride, and then with distilled water; dried and ignited, it weighed 3·63 grs. = iodine 1·951.

It now remained to examine the distilled liquids for iodine, as it existed in these in the free state dissolved in alcohol; they were mixed together, and placed in a counterpoised matrass with metallic zinc. After prolonged digestion, a little water added to facilitate the operation, the iodine was converted into iodide of zinc; the fluids were then distilled off, the iodide of zinc dried at 212° and weighed; 3·35 grs. = iodine 2·6715 grs. were obtained.

Now, if any substitution compound had been formed by the action of the iodine on the quinine, it was probable that hydriodic æther would be produced; if so, it wouldbe found in the distilled fluid; this was carefully examined for this substance, but none detected. Subsequent experiments showed that none could have been produced, or if any, so small a quantity, that its presence would be immaterial, for all the iodine used, with the exception of 2·56 grs., can be accounted for thus :—

21·7375 iodine in the 66·6 grs. of crystals at 32·63 per cent.
  1·0800 iodine in the acid mother-liquids (as crystals).
  1·9510 iodine in the alcoholic mother-liquids (as crystals).
  2·6715 iodine in the distilled fluids as free iodine.

27·4400
  2·56   grs. iodine lost in drying the crystals by expression
          between folds of bibulous paper.

Had a substitution compound been formed, one-half the iodine should have formed hydriodic acid, the other half should have been in the crystalline compound; therefore it is evident to me that no such substitution base can be the result.

One other question arises, Does the iodine exist in the compound as hydriodic acid? Some of the crystals were dissolved in *diluted* alcohol boiling, and starch was added to the hot liquid; instantly an abundant precipitation of the blue iodide of amidine occurred; starch was added in excess, and until no further indications of iodine were evident; the fluid was then separated by decantation, and tested with nitrate of silver; not the least trace of hydriodic acid or any soluble iodide was apparent; similar results were obtained when the crystals were dissolved in hot acetic acid and tested with starch; but if they were dissolved in hot rectified spirit of wine, ·838 spec. grav., there was no evidence of iodine; on cooling, the crystals again formed, the reason being that the chemical attraction of alcohol for iodine was greater than that of iodine for starch, whereas it was less than that of the iodine in the compound. It has therefore been proved satisfactorily that the iodine cannot exist in the compound as a substitution base or even as hydriodic acid.

The iodine separating in the free state so readily upon dissolving the crystals in alcohol or in acetic acid, rendered it a somewhat difficult matter to estimate it correctly. Starch was first used as the precipitant; the resulting iodide of amidine was decomposed by sulphuretted hydrogen, the hydriodic acid produced neutralized by ammonia, then precipitated by nitrate of silver, and the resulting iodide of silver estimated; but accuracy was very far from being attained by this method, in consequence of iodine distilling during the heating of the fluid and solution of the crystals.

At length it was found, that by passing a current of washed and pure sulphuretted hydrogen through acetic acid, in which a known weight of crystals had been placed, and applying heat to the mixture, as soon as gas commenced being evolved, the iodine was converted into hydriodic acid upon its being liberated from the crystals; the decomposition being perfect and the ope-

ration finished, the excess of sulphuretted hydrogen was expelled by boiling, testing with acetate of lead paper occasionally, the precipitated sulphur removed by filtering, washed well with distilled water, and to the filtrate ammonia added, nearly to neutralization, but short of precipitating the quinine; then the solution boiled and the iodine precipitated by nitrate of silver, collected on a filter, washed with distilled water, then with diluted nitric acid to remove any quinine (which falls with iodide of silver even from an acid solution), and lastly dried and fused by ignition in a platina capsule; 25 grs. of crystals gave by this method 15·14 grs. of iodide of silver = 8·1523 grs. of iodine = 32·6092 per cent.

The solution after the separation of iodine was then, together with the washings, treated with acetate of baryta until no further deposition of sulphate occurred; it was boiled to hasten the separation, filtered, washed, dried, ignited and weighed; it gave 7·76 grs. $BaO + SO^3 = 2·653$ $SO^3 =$ sulphuric acid per cent. 10·612.

The liquid after the separation of iodine and sulphuric acid was then acted on, first by sulphate of ammonia to remove excess of baryta, then with hydrochlorate of ammonia to remove the excess of silver.

To this fluid, concentrated by evaporation to about 3 fluid ounces, was added ammonia in excess; an immediate deposition of alkaloid was the consequence. Æther was now added in sufficient quantity to dissolve the alkaloid, the supernatant ætherial fluid was decanted into a counterpoised flask, the operation being repeated as often as necessary; the ætherial fluids mixed were then distilled; the residue, dried at 212° Fahr., weighed 7·533 grs.

The aqueous and ammoniacal solution, upon evaporation to dryness in a water-bath, again treating with æther as long as necessary, and distilling as before, furnished a second quantity of alkaloid, weighing, after drying as before, 3·14 grs.

Then 7·533 + 3·14 = 10·673 alkaloid equal to 42·692 per cent. This analysis, therefore, accounts for—

		Per cent.
Iodine		32·6092
Sulphuric acid		10·612
Alkaloid		42·692
		85·9132

The loss of 14·0868 was probably water of crystallization, but it now became necessary to perform an analysis to make this point certain; after several attempts, the following process was adopted, and furnished correct results.

Having arranged an apparatus for preparing a current of dry hydrogen gas, the stream was passed through a flask containing

iron filings; these were then heated red-hot, the organic matters were decomposed and reduced to pure carbon, and the oxide of iron was reduced to the metallic state; when the gas issuing from the exit-tube of the apparatus burnt with a steady, yellow flame, the operation was discontinued; at least the spirit-lamp was removed and the iron allowed to cool, still in an atmosphere of dry hydrogen gas, and when cold, removed and well secured in a small stoppered bottle.

To the same apparatus for generating the dry hydrogen was adapted a counterpoised test-tube (*a*), and to the exit-pipe from this was connected a tube containing chloride of calcium, this tube, with its contents, being accurately counterpoised.   Into the counterpoised test-tube (*a*) was placed a mixture of 10·2 grs. of the crystals previously dried at 212°, rubbed up in a mortar with 50 grs. of the purified iron filings; the mortar was wiped out carefully by 20 grs. of the same iron, and this also inserted in the tube, a layer of pure iron filings placed over the whole, and the tube, with its fittings, again weighed.

This part of the apparatus was then placed in a flask containing a solution of chloride of zinc—destined to act as a bath: the whole apparatus having been satisfactorily adjusted, heat was applied to the bath, and gradually raised until the chloride of zinc ceased to give off any water, and of course fused; this must have been 420° Fahr. or more.

In this operation the crystals were decomposed; the iron seized the iodine as fast as it was liberated; the quinine retained the sulphuric acid; and the current of dry hydrogen gas carried over the aqueous vapour to the chloride of calcium tube, where it was retained: the increase in weight was 1·44 grs.: then as 10·2 : 1·44 :: 100 : 14·1764 of water.

This method was also adopted as a means of estimating the iodine, but for this purpose the chloride of zinc bath was not employed, as a more perfect decomposition was then necessary; the mass in the test-tube (*a*) was lixiviated repeatedly, as long as any iodide of iron was dissolved; this was at once filtered into a solution of nitrate of silver, iron filings being kept in the filter to avoid decomposition; the filter was repeatedly washed with boiling distilled water, and of course the washings added to the previous liquid; the resulting mixture of iodide of silver with the oxides of iron was thrown on a filter, washed with hot diluted hydrochloric acid as long as any iron was removed, then with ammonia to remove any chloride, and then with distilled water; dried and ignited, it weighed 6·00 grs. = iodine 3·1453 = per cent. 31·453, corresponding very closely with that previously obtained.   A second analysis, specially directed to the estimation of the sulphuric acid, gave 10·844 per cent. as the result.

Therefore we now have found—

	A.	B.		Calculated.	Per cent.	At.
Iodine .	. 32·6092	31·453	124	Iodine .	. 32·63	1
Sulph. acid	10·612	10·844	40	Sulph. acid	10·52	1
Alkaloid	. 42·692		162	Quinine	. 42·63	1
Water .	. 14·1764		54	Water .	. 14·2152	6
	100·0896		380		99·9952	

These results correspond very closely with the formula $(C^{20} H^{12} NO^2 + I) + SO^3 + 6HO$; and as it has been previously proved that the base is not a substitution compound, it only remains to consider it as a salt in which iodine is superadded to the base quinine without interfering with its basic properties, however much it may alter its chemical characters. Experiments have been instituted to produce this iodo-quinine in an isolated state, but hitherto without success,—the nearest approach hitherto made is by the action of ammonia at 60° Fahr.,—by carefully triturating the polarizing crystals in the strongest Liquor Ammoniæ during half an hour, separating on a filter, washing with cold distilled water, and carefully drying the reddish-yellow mass produced: this contained *nearly* all the iodine and quinine, but the ammoniacal solution contained sulphate of ammonia; some of the resinous compound dissolved in it, together with about 11·0 per cent. of hydriodate ammonia. The resinous mass was treated with diluted sulphuric acid, in order to attempt to produce the polarizing crystals again; they certainly were reproduced, but not in a satisfactory manner; some other compounds were also produced, the formation of which cannot be accounted for in the present condition of the question.

The alkaloid separated by the previously detailed analysis was then examined. From it was first made the disulphate; this differed materially from disulphate of quinine, both in its crystalline form and its solubility; it would dissolve in about three times its weight of water at 212°, crystallizing in radiating plumose tufts, very similar to acetate of morphia in appearance.

From this disulphate were reproduced the polarizing crystals very readily upon submitting it to the same operation as was originally used, namely, solution in acetic acid, and then adding an alcoholic solution of iodine to the heated fluid; on cooling, the green crystals deposited, having their original extraordinary properties. The disulphate of the alkaloid differed as much in its optical as in its chemical characters from quinine; assimilating itself in the former to the disulphate of quinidine (β-quinine), whereas the pure alkaloid much more resembled quinine in its chemical characters, as it is soluble both in alcohol and in æther, but crystallizes from neither with facility; some slight appearance of crystallization is obtained by

exposing an alcoholic solution to spontaneous evaporation in a test-tube; around the edge of the liquid a thin radiating plumose crop is produced, being more distinctly acicular than the disulphate. It is therefore not quinidine (β-quinine), but assimilates probably to that variety of quinine recently called γ-quinine, a monohydrate of the organic radicle $C^{20} H^{12} NO^2$, of which α-quinine is the tri-hydrate, and β-quinine the bi-hydrate. However, further researches are necessary to establish this fact; for the present we are justified, from the reproduction of the polarizing crystals from the alkaloid separated from the green polarizing compound, in considering that the alkaloid quinine enters into the composition of the crystals, but in the character of an iodo-base; *not* a substitution base, as has been previously shown, but a compound analogous in its constitution to iodo-codeine, dicyano-codeine, cyaniline, cyano-toluidine and cyano-cumidine, all of which are compounds *not* belonging to the series of substitution products: this, if correct, is a remarkable fact, and worthy of verification by a more elaborate investigation.

It is necessary to correct an error into which I inadvertently fell in my last communication, in reference to the optical properties of the disulphate of cinchonine, fig. 11. Pl. IV.; this should have been disulphate of the alkaloid quinidine (β-quinine).

Since the publication of my last communication, I have succeeded in producing and mounting an artificial tourmaline, large enough to surmount the eye-piece of the microscope, so that at the present moment I am perfectly independent of the tourmaline or Nichol's prism in all my experiments upon polarized light; and the brilliancy of the colours is much more intense with the artificial crystals than when employing the natural tourmaline; as an analyser above the eye-piece, it offers some advantages over the Nichol's prism employed in the same position, for it gives a perfectly uniform tint of colour over a much more extensive field than can be had with the prism.

32 Old Market Street, Bristol.
June 11, 1852.

XXVIII. *On Copper Smelting.* By James Napier, *F.C.S.**

[Continued from p. 59.]

Assaying of the Ores.

THE first object of the assayer, like that of the smelter, is to separate the earthy matters contained in the ore from the metallic portion. But experience has taught, that if the copper

in the metallic portion or *mat* exceeds 50 per cent., the slag or scoria obtained will contain some copper, and there will therefore be a loss. The mat should not contain more than 40 per cent. of copper in order to get clean slag. The assayer therefore arranges his samples according to their quality as determined by the eye. Those full of *mundic,* having much sulphur and iron, and containing arsenic, are kept apart, in order to be subjected to a dull red heat for a short time to expel a portion of these impurities before adding flux.

A portion of the sample to be assayed is weighed off. The assayers have special weights divided into 100 parts, termed cents or centners, with $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$th ; 100 parts or cents are generally taken ; and if no excess of sulphur or arsenic be present to require a slight roasting previously, the ore is mixed with a quantity of flux composed of lime, borax, fluor-spar, a little salt and nitre ; occasionally a little soda or potash is used, and ground window-glass. The nitre is termed the operating flux, as it purifies the ore by supplying oxygen to the arsenic and other impurities present ; the quantity of the flux added is not very precise, generally about twice the weight of the ore used, but sufficient to cause the perfect fusion of all the silica present. The flux and ore are intimately mixed and put into a clay crucible having a clay cover, then placed in a furnace and brought to complete fusion, in which state the mass is kept for about ten minutes ; the whole should be perfectly liquid, and should exhibit no effervescence. The crucible is then removed from the fire, and the contents poured into an iron ladle and allowed to cool : some operators immerse the ladle and contents into cold water. When taken from the ladle, the metallic portion forming the *mat* is found as a button at the bottom, the slag or scoria on the top ; they are separated, and the slag carefully examined for any metallic particles before being thrown away ; but, as above observed, if the mat contain less than 40 per cent. of copper, the slags are generally free. The contents of the crucible, instead of being poured into a ladle, may be allowed to cool in the crucible, which is afterwards broken and the slag and mat separated. The crucible being seldom fit to use again, no loss is occasioned ; however, it is seldom practised by the Cornish assayers.

The regulus or mat, when separated from the slag, is finely ground and put into a clean crucible, taking great care that none of it is lost ; it is then placed in a slow fire, and gradually brought to a dull red, the powder being stirred constantly with an iron rod to prevent it caking ; the point of the rod is examined from time to time to see whether there is the slightest tendency to cake, which is evinced from the particles adhering to the rod, in

which case the crucible must be instantly removed from the fire and the heat lowered. After a great quantity of the sulphur is volatilized, there is less tendency to cake; the heat may then be increased gradually to a bright red, and continued until all the sulphur is expelled, which is ascertained by taking out the crucible and holding the head cautiously over it. The success of the operation depends upon the perfect calcination of the regulus; should any sulphur be left in the powder, there is great risk of the copper not being all got in the fusion. When calcination is complete, the crucible is allowed to cool, and the contents mixed with from one to two times its weight of black flux (according to its richness in copper), and its weight of ground crown-glass and borax mixed in about equal parts. The whole is put into the same crucible as used for calcining, and a layer of borax spread over the surface; the crucible is placed in a furnace, and the heat raised until the whole fuses: the more intense the heat in this operation the better. It is kept in fusion for about ten minutes. The mass should not only be fluid, but there should be no effervescence or ebullition. When removed from the fire, the contents are either rapidly transferred to an iron ladle, or allowed to cool in the crucible, which is then broken; in either case a button of metallic copper is found at the bottom. The scoria from this operation is ground fine, and carefully examined for metal; and if any particles are seen, it has to be re-fused with a little more flux, and the small button or prill obtained added to the first.

The copper obtained is often brittle and hard, and has consequently to be refined; this is an operation requiring some little experience to perform properly. The metal is put into a clean crucible with a very small portion of black flux, or borax, and brought to fusion; the fused button should not be covered with the flux; there is then thrown upon the fused metal from time to time small portions of refining flux, made by mixing together

> 3 parts nitre,
> 2 parts argal,
> 1 part common salt,

and igniting them in the same way as in preparing black flux. When the metal is very impure, a little more common salt is added. The addition of this flux is made until the button appears to clear easily from a red skin over it: this operation requires time; a little borax is added just previous to taking from the fire. The metal thus refined should be ductile, capable of being hammered thin without cracking on the edge, and when broken, the fracture should be fine-grained, and have a silky lustre. The scoria from this refining operation generally

contains a little copper, and is ground up and fused. This is generally done along with that from the reducing operation, and the prill from the two added to the assay.

When the ores to be assayed are rich in copper, such as subsulphurets, oxides, and carbonates, there is generally added to the first fusion, when separating the gangue from the metallic portion, a quantity of sulphur, in order to form a regulus or mat of the desired quality, as stated above, which is proceeded with as already described. To fuse carbonates and oxides with sulphur, and then be at the labour and cost of calcining to get quit of the sulphur again, seems ridiculous. And when these ores have little earthy matters in them, it is unnecessary; but when, as in many cases, there are upwards of 50 per cent. of earthy matters present, in fusing these with a reducing flux, the liability of the oxide of copper to combine with the silica and remain in the slag is so great, that it is found better in practice to take the apparently longest method in order to obtain the most correct results.

The above is a mere outline of the general method by which the ores of copper are assayed. The following may be said to be a general rule followed in suiting the fluxes, &c. to the kind of ore after assorting.

Fluxes for different Ores.

Yellow sulphurets .
- 1st *heating* with a little nitre, quantity depending upon the sulphur and arsenic in the ore.
- 2nd *fusing.* Fluor-spar, lime, borax, salt, a little argal, quantities according to the earths.

Gray sulphurets .
- 1st *heating.* Nitre, small proportion.
- 2nd *fusing.* Borax, lime, salt, fluor-spar, and a little argal.

Black sulphurets . . Same as above.

Red and black oxides and carbonates . .
- 1st *fusion.* Lime, fluor-spar, and sulphur.
- 2nd *fusion.* Borax, salt, lime and argal, or black flux.

Native copper . . . Only refined.

Ground tartar or argal is often used instead of black flux. The black flux is prepared by mixing intimately—

2 parts nitre, and
3 parts tartar or argal

in an iron mortar or other vessel that will stand heat, and inserting into the mixture a red-hot iron or red cinder. When rapid

conflagration takes place, cover with a tile or any convenient article till the burning ceases; then grind what is left, and keep in a stoppered bottle as it is liable to deliquesce.

By the results of the assays both the buyer and seller of the ores are guided, and the regular agreement of the various assayers in their results is a proof of general accuracy. When, week after week, not less than a dozen assayers, every one separately assaying the same sample of ore two or three times over, are all found to agree within $\frac{1}{14}$th of a per cent., it must be a source of confidence to all parties. Nevertheless many of the assayers know as little of the principles of their operation as the miner or seller; consequently the whole process, as it is practised, is a mechanical operation, and one that has undergone little or no change these two centuries, as will be evident by a few extracts from the works of the celebrated *Lazarus Erckern,* published in this country in 1683. Indeed so accurate are his processes and descriptions, that an edition of his works, with our improved nomenclature and apparatus, would be a valuable addition to the library of every chemist and metallurgist.

" *To make Flux to prove Copper Ores.*

"Take two parts of argal and one part nitre, grind them small and mingle them, and put the whole into an unglazed pot, and put a little live coals in it, when it will begin to burn; when the burning gives over, put away the coals, and grind the flux and keep it in a warm place; if set in a cool or moist place it deliquesces. This flux is to be used to good copper ore; but for flinty or other ores hard to melt, this flux is too weak of itself; there must be something additional added."

" *How to prove easily flowing (melting) ores.*

"Rich good copper ores (not flinty and speiry) are proved thus: grind the ore small, and weigh two centners (200 parts), and put them in a crucible with three times as much of the before-mentioned flux well mixed; then cover this with a layer of common salt a full finger thick, press it down, cover the crucible luted with clay to prevent coals falling in or the contents flowing over; place the crucible in the fire or oven, then cover with coals about an handbreadth high, and blow through the hole under the grate that the wind may go alike round the crucible until in perfect fusion; let it stand awhile in the fusion, then take the upper fire off and lift out the crucible, and set it on a plain tile that the grains of copper may settle. When cool, the crucible is broke, and a button of copper is found at bottom. You must observe in proving, the heat be not too high, for the copper will burn and drive itself into the slack (slag). If the

slacks are red, the heat has been too high ; but if brown, the proof is good."

" *How hard-flowing Copper Ores are proved.*

" Hard-flowing ores are not to be proved as the smooth, but in another manner. Thus, take the ore, beat it as small as the seeds of hemp, weigh two centners and put into a crucible, and give it a very gentle heat that it begin to roast itself, stirring it with an iron rod, else the ore will turn to ashes (cake) and not roast. When it is stirred the first time, give it a little stronger fire that it may glow well, then lift out the crucible and let it cool ; it is then put back into the fire, and kept there until it has done smoking and smells not of sulphur. It is now ground a little finer, but not so fine as flour ; roast it again until it stinks no more of sulphur, stirring it with the iron rod to prevent caking. When cool, it is again ground as fine as can be, and roast once more until quite dead, that is, till it has no sulphurous smell, when it is ready for the proof. Divide the whole into two parts, so that should the first fail, another can be done, or two may be done to have a surer proof. One part is put into a crucible, with three times as much of the before-mentioned flux and some flowing *glass gall* (a flux mostly oxide of iron) well mixed, and cover it with common salt, as before mentioned ; lute it over with clay, and fuse for a considerable time ; take out the crucible and break it, when a button of copper of quality according to the nature of the ore will be got, from which a right proof of the ore will be obtained."

These short extracts upon copper ores will verify the above remarks.

From the fact of the manufacturers of copper obtaining a greater quantity of copper from the ore on the large scale than is indicated by the assay, the source of this has been sought for in the above method of assaying not giving accurate results. We have tested the slag of the Cornish assayer, and have almost invariably found it to contain copper, but not more than the slag obtained from the smelting operations ; the cause of the discrepancy between the assayer and smelter will be noticed in another paper. In the mean time it has been suggested to be owing to the assayers using fluor-spar as a flux ; but this substance the smelters also use as a flux.

The flux we use for fusing, when assaying sulphurets, and which gives us slags without a trace of copper, is composed of—

> 1 part dried borax.
> 1 part slaked lime.
> 1 part oxide of iron.
> $\frac{1}{4}$ part nitre.
> $\frac{1}{4}$ part common salt.
> $\frac{1}{16}$ part of charcoal dust.

Mix intimately 400 grains of the ore with twice its weight of the above flux, and fuse in a crucible for ten minutes; if the ore is very siliceous, a little more of the flux is used. When the crucible is cold, break it, and the mat or regulus is found at the bottom; the slag is a glassy homogeneous mass. The regulus is calcined and proceeded with as before described.

Besides these methods of assaying by the dry way, or fire, there are various methods practised by acids, or what is termed the wet way. The first is the precipitation of the copper by caustic potash: take 25 grains of the ore, and digest or boil for an hour in a mixture of two parts nitric acid and one hydrochloric slightly diluted with water; to this add ammonia till the whole smells strongly of that alkali, then pour the whole upon a paper filter, and when the liquid has passed through, wash the contents of the filter by pouring over it water containing a little ammonia: this washing is continued until the water passing through is colourless. The whole liquid obtained is put into a flask or beaker and boiled; when boiling, add some caustic potash or soda, and continue the boiling until the blue colour disappears, and there remains no smell of ammonia; the copper then forms a black precipitate. This solution with precipitate is now put upon a paper filter, and the precipitate washed with hot water until the water passing through the filter ceases to turn red litmus paper blue; the filter is then dried, and the precipitate scraped off the filter into a porcelain crucible; the paper is burned on the cover of the crucible exposed to the air, and the ashes laid on the top of the precipitate; the crucible with contents is then brought to a dull red, and afterwards weighed. The substance thus obtained is oxide of copper, every 40 parts of which are equal to 32 of copper; the result is thus calculated by the common rule of proportion, and the quantity of copper in the 25 grains ascertained. This method is tedious, and requires great care to obtain accurate results; and the results are always higher than can be obtained by furnace operations.

Another method often practised is by digesting in acids as stated above, and, after filtering the blue solution obtained by ammonia, evaporating to dryness, redissolving the dry residue in a little hydrochloric acid, and then diluting with water and adding a piece of iron or zinc, when the copper precipitates in a metallic state upon either of these metals. When the whole is precipitated, the zinc or iron is washed and the precipitate collected upon a weighed filter, washed, dried and weighed: the first washing-water should have a little sulphuric acid added to it, and the washing afterwards continued till no trace of acid is left. With care, very accurate results may be obtained by this method, but it is tedious.

Another and very simple plan is by colour. The operator

provides himself with three bottles or glass tubes of equal dimensions, and a measure graduated into 10 or 100 divisions; a common alkalimeter answers the purpose: 2 grains of pure copper are weighed off and dissolved in nitromuriatic acid, then excess of ammonia added. This solution is made up to two measures exactly of the graduated glass or alkalimeter. Two of the bottles or tubes are filled with the solution, and well-stopped and placed upon a frame. To test an ore by this method, 20 or more grains are digested in nitromuriatic acid, as previously described, ammonia added and then filtered; the blue solution is now tried by filling with it the remaining third tube, or bottle, and placing it between the other two in the frame and comparing the colour; it is diluted carefully until the tint is exactly the same as the tubes outside; the whole is then measured in the alkalimeter, every measure of which being equal to one grain of copper, the per-centage is soon ascertained. A little practice in this method gives tolerably accurate results with expedition; however, they are generally too high to serve as a guide in purchasing the ore. By making up test-bottles of different strengths, such as $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ of a grain, the process is well adapted for testing slag and refuse of furnaces or other products, and should form a part of the operations in every assaying-room or smelting-works, a subject we may have occasion to refer to again.

Similar methods have been recommended, but in which, instead of judging by the colour, a solution of another salt is used that destroys the colour, such as the sulphurets of sodium and potassium; solutions of these salts of such strength are employed, that a given measure is equivalent to a given weight of copper. Mr. H. Parkes has recommended a solution of cyanide of potassium as follows:—"Take a given quantity of pure copper (say, for instance, 10 grains), place it in a flask, and dissolve in nitric acid; add ammonia in excess, and then make it into a bulk of about 2500 grains by measure by the addition of water, although this is not absolutely necessary. Dissolve 1 oz. (avoirdupois) pure cyanide of potassium, free from ferrocyanide or sulphuret of potassium, in 5 oz. by measure of water; filter, if necessary, and place the solution in a well-stoppered bottle till required for use. I then ascertain the quantity of this solution of cyanide of potassium required to decolorize the solution of copper, by taking a given quantity in any graduated vessel, as a burette, and pour it by degrees into the solution of copper, adding the last quantity drop by drop till decolorized. This is very easily perceived, as there is no precipitate to interfere; and the operation is conducted at the ordinary atmospheric temperature. Mark down the quantity required (say 500 grains) by volume. After having established these data, it is very easy to estimate the quantity of copper con-

tained in any ore or cupriferous product, by simply dissolving a
certain quantity (say 20 grains in nitric or nitromuriatic acid),
with the assistance of heat, if required, as in the case of some
sulphurets, the addition of ammonia in excess is necessary; and
if any considerable quantity of iron or alumina was present in
the sample, it should be allowed to digest at a gentle heat, under
ebullition, to make sure that all the copper is taken up by the
ammonia; filter into a flask, wash the precipitate with water,
and make into a bulk of 2500 grains, as when taking the stand-
ard of the solution of pure copper. All that now remains to be
done is to allow it to get cold, and add the cyanide of potassium
until decolorized, noticing the quantity taken. Suppose it
required 400 grains by volume of the cyanide solution; then
from the proportion—500 grs. K Cy : 10 Cu :: K Cy 400 : Cu 8
—the quantity of copper contained in the 20 grains of material
taken for analysis, or 40 per cent. If the ore taken was a sul-
phuret, it is sometimes advisable to filter, in order to separate
the sulphur before adding the ammonia, or else to use a dilute
solution of ammonia, and a gentle heat when digesting, or small
particles of sulphuret of copper might be reproduced, especially
when the precipitate produced by the ammonia is a bulky one."

These processes are complicated, and liable to many sources
of error, and require an experienced chemist for their perform-
ance; while any process to be generally useful to the assayer
or smelter, should be easily performed, and the liability to
errors few.

Depositing the copper from solutions by means of electricity
has also been recommended as a process for assaying. The
process is simply to get the copper into solution by digesting
the ore in acids, diluting and filtering, as already described;
in the filtered solution is placed a porous vessel of unglazed
earthenware filled with a solution of common salt or weak sul-
phuric acid, in which is immersed a piece of amalgamated zinc
or a piece of iron, connected by a copper wire with a small
piece of copper previously weighed, which is put into the copper
solution. The porous cell should be so placed that the piece of
copper be under it. Galvanic action begins between the copper
and zinc, and the copper held in solution is deposited in the
metallic state upon the slip of copper; the action is allowed to go
on until all the copper is extracted from the solution, which may
be known by taking out a drop and touching it with ammonia,
which if copper be present gives a blue colour; or by dipping
into the solution a blade of a knife, which will take a copper
colour if any of that metal remain in solution. When the opera-
tion is finished, the piece of copper is again weighed; the in-
crease of weight is due to the copper that was in the solution.

This process may be variously modified; and when care is taken, gives accurate results, but far too complicated for ordinary use. We shall have occasion to allude to this process again in reference to large operations.

A variety of other methods for determining the quantity of copper in copper ores have been recommended by different parties, all more or less depending upon careful manipulation. We have contented ourselves with describing briefly the leading features of a few of the modifications and new processes which have been advocated.

The next paper will be on the kind of fuel suitable for smelting copper ores.

<div align="center">[To be continued.]</div>

XXIX. *Additions to the late* Mr. T. S. Davies's *Notes on Geometry and Geometers. The Swale Manuscripts. By* T. T. WILKINSON, *Esq., F.R.A.S.*

<div align="center">[Continued from p. 33.]</div>

THE first and second volumes of Mr. Swale's MSS., though of different sizes of paper, are continuations of each other, and comprise in the whole 425 quarto pages of densely crowded matter, entitled "A Miscellaneous Collection of Geometrical Questions; those not original being proposed for the purpose of *generalizing* and receiving *improved* and *original* solutions." In addition to numerous original theorems and problems, these volumes contain diversified constructions, with occasional analyses and demonstrations, to all the principal geometrical questions which had been proposed in the Lady's Diary, Gentleman's Diary, Mathematician, Burrow's Diary, Hutton's Miscellany, The Mathematical Repository, Student, Mathematical Companion, Enquirer, Leeds Correspondent, Playfair's Euclid, Bonnycastle's Trigonometry, Apollonius, and Simpson's Algebra, Geometry, and Select Exercises; illustrated by upwards of one thousand carefully constructed diagrams. Each day's work is generally pointed out by having its respective date affixed; and not a few incidental notices occur which show that he sustained a long and active correspondence with his friends Messrs. Whitley, Nicholson, Shepherd, Leybourn, Davis, Ryley and Lockwood.

"Mr. Crakelt, of Northfleet, Kent," is designated as "an excellent Geometrician" in page 12, when engaged upon one of his problems from Burrow's Diary;—"four curious Theorems" and a statical problem are pointed out on page 90 as having been "sent to Mr. Leybourn, 6th Dec. 1830;" and a printed sheet containing them is inserted as having been received "from Mr. Leybourn, 14th May 1833," who had arranged them as

Questions 553 and 558 of the Repository. By the time he arrives at page 129 his worldly prospects had been blighted by the "dishonest relative" alluded to by Professor Davies; for at the foot of the page he remarks, "these are trifles, but they divert my attention from the dark clouds of my calamitous circumstances;" and a fortnight later, "5th May 1828," he adds, "I do very little now;—the poverty to which I and my children are reduced by plundering villains has prostrated all my enjoyments and hopes. I have slaved for 33 years, and at 53 I am destitute." With how many of our ablest geometers such has been the case we need not here inquire :—*poverty* and *mathematics* seem to be inseparable adjuncts to those of the Lancashire and Yorkshire schools; for amongst them, wherever a genius for the latter has existed in an unwonted degree, the withering influences of the former have almost invariably been more than ordinarily present. It was ever the case with Wolfenden and Butterworth, and fortune has not been more favourable to some of their illustrious contemporaries who still survive.

In page 160 Mr. Swale notices with commendation a solution by Mr. Jeremiah Ainsworth (the grandfather of the gifted novelist, William Harrison Ainsworth), who was long the ablest and favourite contributor to Burrow's Diary; and in a subsequent page, Mr. Richard Nicholson is alluded to with much tenderness of feeling as his "early mathematical associate and an excellent geometrician," who used to meet him "at Mr. Ryley's house in Leeds to converse on mathematics." "I linger," he adds subsequently, "among these problems and sketches as the pleasing though melancholy reminiscences of days for ever gone and of early acquaintances now silent and mouldering in the tomb." Pages 197, 198 are occupied with the demonstrations of several theorems which he afterwards applies (pp. 227–230) to the determination of the general problem on "Inclinations;" but as these were afterwards corrected and extended in a separate manuscript, they need not at present be more particularly described. The subject of Inclinations is again resumed in pp. 233–235, after another method, and two or three different constructions are given to each case; but agreeably to Mr. Swale's usual practice, no demonstrations are added, which is the more to be regretted since the methods are generally different from those in common use. The Maxima and Minima of geometrical quantities occupy pp. 251–257, which are treated with his usual elegance :—from internal evidence I am led to think that a portion of these were intended to follow those in No. II. of the Apollonius, especially since page 252 supplies a correction to Mr. William Smith's elaborate solution to Question 69, No. X. of the Mathematical Companion. Several of these investigations are

well worthy of transcription; but the complexity of the requisite diagrams renders it impossible, whilst the enunciations would be unintelligible without them.   Indeed one of the peculiarities of Swale's geometry is the *complexity* of his diagrams: he almost invariably uses more lines than any other investigator; but this disadvantage is more than counterbalanced by the elegant simplicity of his reasonings, and the vast number of collateral properties which he developes in his processes.   In these respects I know of no geometer who has so nearly equalled him as the late Professor Davies, whose diagrams have frequently presented such a similarity to those used by Mr. Swale as to lead some of the friends of the latter to suppose that Mr. Davies had access to these MSS. long before he even knew of their existence.   The Swale MSS. were not seen by Professor Davies until "October 5, 1850," and he had only time to write the few notes respecting them contained in No. VII. of this series of papers before his progress was arrested by the hand of death.

"A Collection of Problems by the Compasses alone" occupies pages 292–332; but since they are principally of an elementary character, they need not be further particularized.   His son appears to have been very expert in such constructions; for on page 296, after giving an elegant determination of "the centre of a given circle," he adds, "this method, which is more simple and elegant than Mascheroni's, was discovered by J. H. Swale, junior, 19th February 1829."   The solution of isolated problems by ordinary geometry is again resumed at page 332, and is continued throughout the remainder of the two volumes.   In the page just cited, a theorem occurs which appears to have been "sent to Professor Leybourn, 6th Dec. 1830," but did not find its way into the Repository; it furnishes a ready proof of the methods of finding the centre of a given circle already instanced in No. VII. by Professor Davies, and has recently been published as Question 378 of the Educational Times.

The problems on "Tangencies," already mentioned, occur in pp. 383–386; the *fifth* and *sixth* cases being *first* constructed from the principles of the poles of similitude, and *afterwards* reduced to Simpson's principle before stated.   The last portion of the volume appears to have been formed from an earlier manuscript, since a portion of its pages is occupied with the demonstration of several problems relating to poles of similitude apparently deduced from Lawson's translation of Victa's Tangencies, the intervening spaces being filled up with later speculations. In volume III. a few isolated problems from the Diary occur, but the principal portion is occupied with the extension and application of the problems already alluded to as preparatory to the solution of the problems on Inclinations.   He here treats

them under the name of "Original Theorems on the Circle," and appears to have taken more than ordinary pains to complete the demonstrations, and apply the properties deduced to problems corresponding to the theorems and others related to them. The latter portion of this MS. is fully prepared for the press:— *five* theorems are distinctly enunciated, demonstrated, and applied to the solution of *ten* collateral problems under the extended title of "Original Theorems on the Circle, with their use in the determination of some Geometrical Problems." Mr. Davies's attention had evidently been drawn to this remarkable portion of the MSS., for his pencilled autograph occurs at the head of the page, indicating that what follows is a repetition of the preceding theorems and problems. An earlier manuscript appears to have been destroyed in order to form this; and as such is the case with several of the remaining ones, we may reasonably account for the absence of those of earlier dates:—no doubt the contents of those destroyed had already found their way into periodical works and his own Geometrical Amusements, and hence could readily be dispensed with after transferring what was considered worthy of preservation to the manuscripts still remaining. Volume IV., which is endorsed "Geometry and Algebra," commences with a few problems on Loci, which he notes as having been "sent to Mr. Shepherd, 12th Nov. 1830." The general problem of Inclinations occupies pages 2, 89, 91, 93, of which a "General Analysis and Construction" are given; other portions of the volume are devoted to the consideration of isolated geometrical problems from various sources, some of which are noticed as sent to him by Messrs. Whitley and Shepherd; pages 101 to 132 contain a connected series of forty-two geometrical exercises originally compiled as "Lessons" for his son; and amongst these are interspersed solutions of some difficult Diophantine problems, one of which is Question 310 of the Mathematical Repository, where the "Prize Medal" is awarded to "A Lady" (*Mrs. Somerville*) for her solution. An obituary notice of Mr. William Hilton, editor of the Liverpool Student, justly designated by Professor Davies as "a work of rare merit," occurs in page 102, where he is stated to have died "of Apoplexy, at Liverpool, on the 8th of May 1826."

The fifth manuscript volume is entitled "Mathematical Scraps," and commences with a variety of methods for drawing tangents to a given circle, so as to be divided by a line given in position and the point of contact into parts having a given ratio. Pages 20 and 21 contain no fewer than *eight* methods of drawing "through a given point P, a line that shall tend to the point of concourse of two other lines AB and CD given in position;" a problem, for whose ready solution, by draughtsmen, the "Centro-

linead" was expressly invented. The eighth method being remarkably simple is here transcribed:—

"Draw PH, PK, parallel to AB and CD; and the required line PQ will pass through L the point of bisection of HK."

"Some problems (19) on the Maxima" and their application occupy pages 32–45, one of which is noted as "sent to Whitley," and another difficult theorem "to puzzle Shepherd;"—a "new Theorem from Mr. Whitley" and a "Locus from Mr. Shepherd" are merely enunciated whilst the rest are constructed only. The latter portion of the MS. contains solutions to some of the most difficult equations in Bland's algebraical problems, several of which exhibit a ready command of algebraical artifice, and the remainder is filled up with extracts from works having no relation to mathematical subjects.

The title of volume VI. almost sufficiently explains its contents:—it is "Memorandums, Scraps, Mathematical, Poetical, Biographical, Satirical, &c. &c. &c.;" and a slight inspection proves that its designation is not unaptly chosen. A letter to a friend occurs at page 42, in which he complains that "scientific matters are with [him] at such a very low ebb that [he] cannot treat him with any novelties. . . . . . In the mean time" he hopes his friend, "yet in the spring of life, is not unmindful of those ennobling subjects in which [he] had evinced so much ardour and ability. In the midst of *analytical* inquiries," he adds, "be pleased to recollect that Euclid existed 300 years before Christ, and that you are yet nearly a stranger to those Elements which have conferred imperishable renown on their Author and Compiler." Mr. Swale was ever anxious that the ancient geometry should be in the ascendant; nor did he ever omit an opportunity of impressing the beauties of his "Divine Geometry" upon the minds of his younger correspondents. The statical problems alluded to by Professor Davies occur in this volume, but present no difficulties worthy of notice, since they relate principally to the equilibrium of cones, cylinders, and spheres on an inclined plane, most of which admit of easy geometrical constructions. In a letter to Mr. John Whitley, pages 74–77, dated "9th Feb. 1809," he inquires why the "Inscription Problem," as Mr. Davies terms it, has been reproposed in the Companion, since all "must allow that Mr. Lowry's general method in the Repository is sufficiently elegant;" but almost immediately adds, "I have discovered a *general* method of inscribing polygons in a given circle, each side passing through a given point, *which is also applicable to the ellipse.*" The method itself was subsequently published in the Apollonius, No. II. pp. 41–52, and has already attracted the attention of several of our ablest geometers, amongst whom may be mentioned Messrs. Potts, Gaskin and Davies; it is inter-

esting, however, as a fact in the history of this problem, that
Mr. Swale had been in possession of his method and its exten-
sion so many years before its publication.   Pages 82, 83 contain
" an extract from one of [his] mathematical manuscript books "
relating to the premature death of his friend Mr. William Davis,
editor of the Mathematical Companion ; and a fitting tribute is
paid to his memory in a notice of the event, which Mr. Swale
forwarded for insertion in the Leeds Mercury of the following
week.   A few diophantine, dynamical, and other problems occur
in the remaining portions of this volume; but none of them
appear to possess much interest, if we except a dissertation on
the motion of a ball upon elliptical and triangular billiard tables
(apparently suggested by Questions 250 and 270 of the Mathe-
matical Repository), which determines the directions " of impul-
sion so that the ball may, for ever, pursue the same track " on
a triangular table, to be the sides of the triangle of *minimum*
perimeter inscribed in the given triangular table.

The seventh volume is a bulky octavo, which seems from the
repetitions in the paging to have been made up of several smaller
manuscripts.   It bears the same title as the preceding, and opens
with a series of " Lessons " for his son, amongst which are no
fewer than *twelve* " original methods of dividing a given line in
extreme and mean ratio."   They bear the date " 19th May 1833,"
and would seem to have been satisfactory to their author, for he
adds, " we have now done justice to this Ancient Problem."
Many isolated solutions in this volume contain references to the
Geometrical Amusements, and were no doubt intended for
" Parts II. and III." of that valuable work ; others appear to
have been copied from older slips relating to his friend Mr.
Nicholson, which, as is said in the Leeds Correspondent, are
" now brought forward as a sincere tribute of friendship and
respect for the memory of that ingenious Geometrician."   In
page 82 he acknowledges the receipt of a letter from Mr. Ley-
bourn, dated " 21st May 1833," on which he remarks, " Leybourn
was one of my early scientific correspondents, having written to
him 38 years.   I hope yet to spend a week with him at Bagshot.
At the same time I should greet Lowry, another old correspond-
ent, and Cunliffe.   On such an occasion we should take our
harps down from the willows and once more tune them to the
cheering songs of science."

The subject of extreme and mean ratio is again taken up at
page 171, and two other methods of division added to those
already noticed, after which the solution of some rather difficult
surd equations occur which had been sent to him for solution by
Mr. Harding.   Much of the remaining portion of this manu-
script is occupied with solutions of geometrical problems selected

from Simpson's Exercises, the Mathematical Repository, &c., together with a variety of extracts in prose and verse agreeably to the indications of the title-page; but nothing appears to merit particular notice unless it be a short discussion of the different cases of the problem "to determine P in a line MN of any order, so that drawing the tangents PV, PT, to two given circles (A), (B), they shall have a given ratio." The required point P is very elegantly shown to lie in the intersection of the given line with a *given* circle, which Mr. Swale appropriately terms "the circle of tangential ratio," and which obviously becomes the *circle of similitude* when the given ratio is that of the radii of the two given circles. The two MSS. numbered VIII. and IX. have already been noticed by Professor Davies as volumes I. and II. on the Mascheronian Geometry, and need not be further noticed. No. X. is a short paper fully written out for insertion in the third number of the "Apollonius;" it contains *four* constructions and demonstrations to the problem of having "a point P and two parallel lines AQ, BR, given in position, to determine the position of a line PQR, of section, making the rectangle, sum of squares, or differences of squares, of the segments AQ, BR, cut off from the lines given in position equal a given square (V²);" which are designated by Mr. Swale as "diversified solutions to the same problem; or brief introductory Lessons for young Geometricians." The paper is prefaced by a motto which inculcates his favourite dogma, that "*variety* of method, or fertility of resource, is increased *power*," and appears, with one exception, to be the only existing manuscript fully prepared for the press.

The eleventh and last volume in my possession is divided into two parts; the first of which (pp. 5–87) is devoted to the solution of diophantine and other "algebraical inquiries" selected from various authors, and the second part (pp. 281–338) to the consideration of numerous original and selected problems under the title of "Geometrical Amusements, to sooth an incurable despondency." Pages 298–308 contain a discussion of the problem "to determine a point P, in AC, the side of a given triangle ACB, such that drawing PQ perpendicular and PR parallel to the base AB, the ratio, sum, difference, rectangle, sum of squares, or difference of squares, of PQ and PR, may be respectively equal to given quantities;" *four* different constructions and demonstrations being given to each case. The problem partially considered in MS. No. X., as extended to the cases of the ratio, sum, or difference of AQ and BR, occupies pp. 308–316, *four* different constructions, &c. being given to each of the six cases, as in the previous instance. In a similar manner he treats the problem "to draw PQR, through a given point P, to meet AK,

AL, given in position, in Q and R, so that the ratio, sum, difference, and rectangle of AQ and AR, may be respectively equal to given quantities;" each successive variation unfolding new properties of the illustrative diagrams, and affording additional proofs of the extensive powers and ingenuity of their author. A question from the Mathematician proposed " about 1750, by John Turner, who had a school at Heath, near Wakefield, Yorkshire," is considered on pp. 327–8; and a case of " Apollonius on Inclinations " appropriately closes the volume, which, from *internal* evidence and the title affixed to the geometrical portion, most probably contains the latest efforts of Mr. Swale's untiring mind.

I have been the more particular in describing the contents of these manuscript volumes, partly in consequence of their number and extent, but mostly from the extreme improbability of even a tithe of them being ever given to the public. Portions occur here and there, like oases in a desert, which might be selected for publication in a separate form did the taste for the ancient geometry warrant such a proceeding; but since such is not likely soon to be the case, the probability is that an immense mass of Mr. Swale's speculations must ever remain in an incomplete and unprofitable condition,—a notable monument of misdirected energy and useless expenditure of valuable time. His systematic researches on tangencies, maxima and minima, the inscription of polygons in circles and in each other, printed in his Apollonius, afford convincing proofs of how much he was capable when his extensive powers were directed to regular subjects of inquiry; for the elegant methods of research employed in these papers, and the simplicity and beauty of the results obtained, must ever command the admiration of geometers. His fertility of invention and originality of conception were inferior to those of no contemporary geometer; and had he directed those energies to systematic inquiries which he expended in the solution of some thousands of isolated and comparatively uninteresting questions, he might have systematized scattered topics or originated new theories, in which he would have rivalled Carnot in transversals, Davies in spherics and porisms, or Chasles in anharmonic ratio, and have secured for his own name a permanent place in the history of modern geometry.

What will ultimately become of the MSS. is of course beyond conjecture. That they will be almost religiously preserved by his son during his life no one will doubt who is acquainted with the profound veneration he entertains for the memory of a kind and indulgent father; but when we call to mind that a *second* generation has deliberately *burnt* the MSS. left by the Stewarts, and that already much of Mr. Swale's correspondence, &c. has been destroyed by an accidental fire, it may not be considered

improper to suggest that the remaining manuscripts ought to be deposited in some public library, where they would at once be safe and accessible, and like Dr. Simson's *Adversaria* at Glasgow, ever remain an enduring monument of the genius and industry of so devoted a geometer.

Burnley, Lancashire,
June 17, 1852.

XXX. *Theorems in the Doctrine of Combinations.*   *By the* Rev. Thomas P. Kirkman, *A.M., Rector of Croft with Southworth.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I WOULD beg your permission to enunciate the following theorems in your Journal:—

A. With 7 symbols can be formed 21 triads, so that every duad shall be thrice employed.

B. Two distinct systems of 7 quadruplets each can be made with 7 symbols, both exhibiting twice all the 21 duads.

C. A system of 21 quadruplets can be made with 7 symbols, so that every possible duad shall be six times employed.

D. With 13 symbols can be made three different groups of triads, each group once containing all the duads.

E. With 15 symbols different triads can be made, so as to exhaust the possible duads, once, twice, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, or 13 times.

F. With $(12n+3)$ symbols can be formed triads so as to exhaust the duads $6n-1$, or $6n+2$ times; and with $12n+7$ symbols, so as to exhaust the duads $6n+1$, or $6n+3$ times.

G. With 27 symbols triads can be made, till the duads have been all twice employed, or all thrice employed.

H. With $4(3n+1)$ symbols quadruplets can be made, till every duad has been $(2n+1)$ times employed, and this without repeating any triplet.

I. With 4×2^n symbols, quadruplets can be made till every triad has been once employed.

J. Sixteen young ladies can all walk out four abreast, till every three have *once* walked abreast; so can thirty-two, and so can sixty-four young ladies; so can 4^n young ladies.

Croft Rectory, near Warrington,
August 6, 1852.

XXXI. *On the supposed Identity of the Agent concerned in the Phænomena of ordinary Electricity, Voltaic Electricity, Electro-magnetism, Magneto-electricity, and Thermo-electricity. By* M. Donovan, *Esq., M.R.I.A., formerly Professor of Chemistry to the Company of Apothecaries in Ireland.*

[Concluded from p. 138.]

Section VIII.

I HAVE in a former part of this essay made some observations on the evidence afforded by the deflections of the galvanometer, and will now offer some additional considerations on the same subject, believing that it involves the whole question, namely, whether these deflections are metrical indications of the operation of one undecomposable because uncompounded electric fluid or agent, always the same in its nature, varying only in quantity and intensity; or are there conditions of the electric fluid differing from each other in respect of constitution or composition, which, independently of quantity or intensity, are far more powerful than ordinary electricity in producing deflections?

It appears to me, after considering the difficulties in the way of the hypothesis of identity with as little prepossession or prejudice as I am able to view them, that deflections and voltaic phænomena in general are inexplicable according to the doctrine of those who consider them due to the operation of ordinary electricity, acting in great quantity at a low intensity.

I shall now only advert to a few experiments out of a multitude, not in the hope of *proving* anything, but merely with the intention of submitting to the judgement of the reader whether such striking effects can result from an ordinary electricity, so feeble that its agency is altogether supposititious, and affords no evidence of its presence except the phænomenon which is the subject of the doubt. Were it not for analogy, it could not be maintained that in these experiments ordinary electricity is at all concerned; its well-known properties are not recognisable; nor is there any evidence of its presence but the effect on the galvanometer, which it is the fashion to consider obedient to no other power. Were it not for the circumstances that the agent which affects the galvanometer is only transferable through conductors of electricity, it would be as natural to believe that in the following experiments the power concerned is that universal property of matter called chemical affinity; for, so far as the mode of action of the bodies is concerned, the deflections might be viewed as measures of intensity of chemical affinity rather than of electricity. The hypothesis which appears to me most consistent with the facts, and perhaps least repugnant to modern opinions, is to assume, as I have done, the agency of electricity,

—*an* electricity, perhaps, the same in the number of its constituent elements as the ordinary electric fluid; but so different in the ratio of these to each other, or in their mode of combination, as to constitute an entirely different power,—a power or agent, *sui generis*, exerting its peculiar energies, according to the predominant element.

For a length of time after the invention of Schweigger's multiplier, afterwards improved by Melloni, Nobili and others, and called the galvanometer, it was doubted if that instrument could be at all affected by common electricity. At length Colladon proved that it could, by increasing the insulation of the coil of wire; that is, by enabling the coil to carry a greater quantity of the electric fluid than in the ordinary state it is able to do; or in other words, by increasing the intensity of the electricity conveyed. Hence the electricity which the common galvanometer coil is capable of carrying, when placed between the two conductors of an electrical machine, will not without peculiar contrivances be indicated by the galvanometer, no matter how powerful the machine may be.

Yet a bit of copper wire, weighing $\frac{1}{1152}$nd part of a grain, arranged voltaically with a platinum wire almost equally small, and a drop of nitric acid, as already described, caused the needle to whirl round the circle three times. Does the particle of copper, scarcely visible, generate more electricity in an instant of time than the powerful electrical machine can supply? Colladon's experiment proves that intensity is the condition of common electricity which causes deflections; and the sparks obtainable from the coil, while the machine is in action, prove the high intensity of the electricity contained in it,—an appearance totally out of the question in the case of the atom of copper. In opposition to these well-ascertained facts, can it be consistently maintained that quantity is the effective condition? Admitting for a moment that it is so, why does not the singly-insulated coil of the galvanometer act on the needle when it is receiving torrents of electricity from a powerful electrical machine? Is the quantity insufficient? if it be, why does that insufficient quantity become sufficient when the insulation of the wire constituting the coil is doubled? If it be answered, that it can now carry the charge of electricity necessary to produce deflection, a greater quantity being retained by the double silk than could have been confined by a single one, I reply, that this is a plain acknowledgement that it is intensity which acts, a state which the atom of copper cannot, and is admitted not to confer; and if the wire-coil with single silk was incapable of carrying the necessary quantity of electricity from the machine to produce deflection, why is it capable of carrying it from the atom of copper? But

above all, why should frictional electricity be affirmed to be sufficient in point of quantity in this special case, when universally it is declared that it is inadequate to produce voltaic effects on account of the smallness of its quantity?

I do not foresee what answer can be given to these questions, or how the facts are reconcileable, while the opinion is entertained that it is electricity, taken in the common acceptation of the word, which causes deflections.   But if we distinguish the agent excited in the common electrical machine from that which is effective in the voltaic series, the difference consisting in the ratio of the constituent elements of the two fluids, then the deflection in one case and non-deflection in the other become more intelligible.

I will now state a few of my experiments: but it is necessary to premise, that in most of them the action was so transitory, owing to the peculiar circumstances, that there was no permanent deflection, and the number of degrees to which the swing of the needle extended, at its first start, was the only measure which could be observed for comparison.

A glass tray being half-filled with concentrated commercial sulphuric acid, a plate of platinum was laid in it, and connected with one of the binding-screws of a galvanometer by means of a platinum wire.   A stick of caustic potash was fastened to the end of another platinum wire by several coils carried round it; the other end of this wire was connected with the other binding-screw of the galvanometer.   In a few moments the potash had attracted sufficient humidity to become a conductor; its end was plunged into the sulphuric acid, but not so deeply as to touch the platinum plate lying in the bottom.   The needle whirled completely round, and with great velocity.   A second dip had no effect; but perceiving that this was caused by a coating of bisulphate of potash, and having washed it off by a plunge into water, a repetition of the dip into the sulphuric acid produced a new deflection.   After again washing it, the whole stick of potash was plunged on its side; the needle again whirled round with great force.

In order to discover how much of the effect was due to the platinum plate and wire, I removed the stick of potash, and dipped the platinum coil to which it had been fastened into the sulphuric acid; the deflection amounted to $3°$; and in another trial it did not stir.

Becquerel made experiments with acid and alkaline solutions by a different method, and produced deflections which he attributed to electricity.   How, in my experiment, the infinitesimal quantity of electricity generated could whirl the same needle round with violence, which refused to stir when a most powerful

electric machine poured torrents of electricity into its coil by direct communication, is beyond my comprehension, unless, as I have already suggested, frictional electricity contains the minimum and voltaic the maximum of the effective elementary constituent which produces these deflections.

The next experiment was with nitric acid and potash; but in this case, nitric acid being a better conductor, it was not necessary to place a plate of platinum in the bottom. A glass capsule containing commercial nitric acid was connected with one binding-screw of the galvanometer by means of a platinum wire. Another platinum wire, proceeding from the other binding-screw, was well-connected with one end of a stick of caustic potash. When the potash was sufficiently moistened by the atmosphere, its other end was made to touch the surface of the nitric acid: the needle instantly started 130° westward. The same wire detached from the stick of potash and washed, when dipped in the acid, only produced a deflection of 2°, and even that was eastward. Even dilute nitric acid produced considerable deflection with potash.

A strong solution of caustic potash touched by a platinum wire, carrying on its end a bit of platinum foil dipped in sulphuric acid, caused a deflection of 10°.

I fused some caustic potash in a capsule of virgin silver connected with one of the binding-screws of the galvanometer: from the other binding-screw proceeded a silver wire tipped with about a quarter of a grain of sulphur melted on it. The fused potash being touched with the sulphur, a very curious process of combination took place, and the needle whirled round four times with great velocity. On trying a new silver wire without any sulphur, and touching the surface of the fused potash with it, there was not the slightest effect on the needle.

By means of the instrument called the Russian furnace, I heated one end of a strip of sheet copper bright red-hot, its other end being connected with one of the binding-screws of the galvanometer. A copper wire proceeding from the other binding-screw had a copper knob on its end, on which was fused a blob of sulphur; on applying the sulphur to the white-hot copper strip, the needle whirled round twice. To prove that this was not the effect of thermo-electricity, I connected the ends of two copper wires with the binding-screws of the galvanometer, one with each, and brought their other ends into contact by a slight twist. The flame of a spirit-lamp was applied to the twisted junction of the wires: when red-hot there was thermo-electric deflection; but this at length ceased, and the needle returned to the meridian. The lamp being still retained in its place, a drop of burning sulphur was let fall on the junction, when a

considerable whirl of the needle took place.   In further proof that the whirl of the needle twice round was due to the chemical action of the sulphur on the strip of copper, it is enough to observe, that, on repeating the experiment without the sulphur, the deflection was trifling.

Two silver wires, treated in the same manner in the flame of a spirit-lamp, one being topped with sulphur, produced momentary deflection of 90°.

A thin iron wire, connected with the galvanometer, was heated in the Russian furnace, and being touched by a similar iron wire, also connected with the galvanometer, with a particle of sulphur adhering to the point of contact, the needle started off 182°.

Some nitric acid in a glass capsule was connected with one of the binding-screws of the galvanometer by a platinum wire; another platinum wire was connected with the other; and its other end twisted into a knob was first dipped into a little spirit of turpentine, and then into the nitric acid; there was an immediate deflection of 40°.

An iron wire, maintained at a red heat in a spirit-lamp, was touched at the point with the point of another iron wire holding a particle of iodine, both wires being properly connected with the galvanometer; the flame of the lamp became green, and the needle started off 100°.

A rod of grain tin, heated in the spirit-lamp to its melting-point, was touched with a particle of sulphur on the point of a thin platinum wire; there was a momentary deflection of 56°.

A thin rod of antimony and a zinc wire were heated in the Russian furnace to the point, when both metals melted at the ends: the zinc wire in melting bent down, but still retained its continuity, and touched the melted drop of antimony still adhering; the needle instantly whirled round entirely.   This is a difficult experiment to succeed in.

A slender rod of antimony was heated in the flame of the Russian furnace; and just as it was in the act of melting at the point, the drop was received on the end of a rod of grain tin also in the act of melting: the needle whirled round.   If the metals be not kept free from oxide in these experiments they will fail.

A ribbon of sheet copper was heated to a bright red in the Russian furnace, and a rod of grain tin was heated at its end to the melting-point; a drop from the tin was allowed to fall on the white-hot copper, the connexion of the drop with the rod of tin being still maintained; the needle started off 130°, and in another trial it traversed the circle entirely.   Combination seemed to have taken place superficially, for the copper was whitened at the place of contact.

It appears to me, that the three last experiments are sufficiently

distinguished from instances of thermo-electricity by the circumstance of fusion and combination.

There is an experiment of Schönbein which seems very difficult to reconcile to the electrical hypothesis. He says, that if some yellow solution of cobalt be poured into a glass tube bent into the form of the letter U, and if a platinum wire, proceeding from the galvanometer, be immersed in each limb of the tube, it will be found that on heating the liquid in one of the limbs until it becomes blue, a stream of electricity will move from the cold to the heated column of liquid, the strength of the current increasing with the difference of temperature between the two limbs. In one case Schönbein obtained a deflection of 40°. He obtained similar results by heating certain acid solutions; and he showed that these are not cases of thermo-electricity, as might at first view be supposed, but that the effects are attributable to the chemical change occasioned in one of the columns of liquid by the action of heat*. Many experiments of this kind have since been made by others.

In the experiment with solution of cobalt, it is very hard to conceive how a current of electricity could be generated in two parts of one homogeneous, uninterrupted, and excellent liquid conductor. The chemical action described by Schönbein is internal; it is not exerted by the liquid on a second substance, but on itself, within itself; the platina wires are mere conductors. The conditions deemed necessary for electrical disturbance are not present. The experiment, indeed, might be adduced as a case wherein mere chemical action, without any electricity, produced deflection. But if electricity did act, it could scarcely be of any other kind than that described in the beginning of this essay, consisting of different constituents, the deflecting element greatly predominating.

In connexion with this experiment of Schönbein, it should be recollected that the charge of Colladon's Leyden battery of 4000 square inches, could only produce an average deflection of 20° or 30°, unless with an intense condensation of electricity, and then he obtained but 40°, the same as Schönbein. Now, if Faraday's law be applied to this case, the quantity of electricity in Colladon's and Schönbein's experiments must have been the same. Can imagination assist us in conceiving, or reason warrant us in believing, that when a small tube containing a little solution of cobalt is heated at one end, a quantity of electricity passes through it equal to the most intense charge of a Leyden battery of 4000 square inches, imperceptible to all else except the galvanometer, and acting on that but feebly?

In my experiments, it is equally difficult to conceive that such

* Poggendorff's *Annalen*, 1838, vol. iii. p. 270.

powerful deflecting effects could have been produced by the cause commonly assigned, when it is considered that the galvanometer which suffered these deflections, when similarly connected with a powerful electrical machine, proved insensible to its current. In the experiments wherein fused caustic potash or heated metals were touched with melted sulphur, there was not more than a quarter of a grain of the latter, scarcely the half of which acted, yet the needle sometimes whirled round four times[*].

It appears very difficult to comprehend these results, which are but a few out of many obtained by me, unless it be admitted that in the phænomena called voltaic, the agent cannot be identical with that which produces the effects of ordinary electricity. By supposing that certain elementary forces, which constitute the electric fluid, exist together in combination while that agent is in its natural state of equilibrium; and that according to the circumstances under which it is excited into a state of activity, the elements present themselves either in their natural state of combination, or more or less altered in their ratio, and therefore in their properties, the phænomena receive an explanation, as I think, less embarrassed with difficulties, but still it must be admitted with quite a sufficiency of them.

We know that the electric fluid occasionally evinces variable properties according to the mode of its generation; and this variation agrees with the notion of difference of ratio of the constituent elements such as has been here assumed. It may be expedient to advert to a few of these differences of properties. Water is an excellent conductor of the lowest intensities of common electricity; but it is declared on all hands to be not a good conductor of voltaic electricity, and Sir H. Davy says that to such low intensities it is an absolute insulator. Common electricity is remarkable for the distance through which it strikes; a spark of fifteen inches long may easily be obtained from a powerful electric machine; but Mr. Gassiot found that a nine-gallon Leyden battery, charged by a water battery of 1024 pairs of plates, could only project a spark to a distance of $\frac{1}{3000}$th of an inch, and sometimes $\frac{6}{3000}$ths. For a long time it was doubted if voltaic electricity could project a spark at all through air, unless when the poles are gradually withdrawn from contact in a vacuum. Voltaic electricity overcomes the most powerful chemical affinities; none in fact can withstand its influence, although it would merely cause gold leaves to diverge a little. But common electricity, possessing the highest dynamic powers, can only overcome comparatively the weakest affinities. One is powerfully magnetic; the other very little so. One, in order to produce its effects, invariably requires the operation of its two poles; in

[*] The experiments of Cumming should also be considered.

the case of the other, one is sufficient. The magneto-electric machine, if connected with the galvanometer by means of stout copper wires, long enough to place the needle out of reach of the powerful influence of the combined magnets of the machine, will cause no small deflection when the coils are made to revolve rapidly. But if, removing the galvanometer, the same arrangement be connected with the insulated gold leaves of a differential electrometer, the effect on them is barely observable. Here then we have an electric fluid in operation quite different from ordinary electricity, on account of its much greater effect on the galvanometer, and its trifling influence on the electrometer; while the current resembles that of a voltaic series, on account of its chemical and deflective powers, although no chemical action is concerned in its production. Yet the deflective power of magneto-electricity is far weaker than the electricity in many cases produced by a single voltaic combination in which chemical action is taking place, and which nevertheless would not be capable of producing decomposition of water, although magneto-electricity does it with such facility. M. Lamé observes that thermo-electric currents are distinguished from voltaic and magneto-electric currents by their being much more difficultly transmissible through liquids *. It is also worthy of notice, that no power of common electricity passed through the coil of the electro-magnetic apparatus is capable of giving a shock, although the electricity of a pair of zinc and copper plates of an inch square, or even a pair of zinc and copper wires, is adequate to that effect even in a violent degree.

So different and independent of each other are the electric and voltaic agents, that the voltaic spark may be taken through the electric, each retaining its characteristic appearance. And if a fine platinum wire be made red-hot by means of a voltaic battery, a current of common electricity may be passed through it, and drawn from any particular part of the wire in its own proper form of sparks. The *aura electrica* produced will cool the wire when sparks are not taken; but when they are drawn, the wire on each side of that spot will be as much incandescent as ever.

Not the least remarkable difference of properties between the electricity evolved by a voltaic combination and that by a frictional machine, is the facility with which the latter is conducted by all metals, and the obstruction which is experienced by the former in passing through some. An easy mode of observing this difficulty of conduction, is by means of an electro-magnetic apparatus of the construction at present used for medical purposes. If the triad, consisting of one platinized silver plate and two zinc plates, be connected with the coil by twelve feet of

* *Cours de Physique de l'École Polytechnique,* iii. 286.

rather stout iron wire, the shocks received by applying the thumb and little finger to the two binding-screws when the slide is on the seventh pin will be trivial; but change the iron for copper wire of the same thickness, and the shocks will be intolerable. Either of these wires, or of any other metal, would without the least obstruction conduct the lowest intensity of common electricity that can be produced.

Conclusion.

Such are a few of the objections which have occurred to me in considering the explanation of voltaic phænomena. I now conclude this essay, calling to mind an observation of the eminent philosopher whose name I have been compelled to introduce more frequently than I could have wished. He says, " as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide who is in the right*." Should any one hereafter think it worth his while to prove that my opinions are mistaken and my objections groundless, I shall nevertheless reap a valuable reward by having been instrumental in obtaining explanations of what appeared to be incongruities and contradictions, until reconciled and harmonized. My object in questioning doctrines so generally accredited, has been to suggest, that, in the induction of our theory of voltaic electricity, we have been misled by a supposed fundamental principle handed down to us by our original inquirers; and that, so long as the electric fluid is viewed as an uncompounded agent, there is little probability of arriving at a just comprehension of its phænomena.

And now, in conclusion, I have only briefly to recapitulate the objects of the foregoing essay: they are intended to prove that the agent in electric and voltaic phænomena are altogether different, not in their elementary constituent principles (assuming that they consist of such), but in their ratio and mode of combination; so different in these respects as to constitute agents which may be considered *sui generis* as much as any of the various compounds of elementary matter known to chemists, which, identical in elements, in no other respect resemble each other. If all this be true, voltaic phænomena are not produced by what is called electricity.

I have been compelled by the objects of this essay to comment on the opinions of Professor Faraday fully and freely. He himself has declared, that up to the time when he undertook to examine the question of the identity of the agent in electrical phænomena, that doctrine had not been fully established. I

* Experimental Researches in Electricity, vol. ii. p. 266.

always felt a strong conviction to the same effect; and, anxious to discover if *his* investigations had demonstrated the truth of the contested proposition, I devoted my attention to them particularly, I hope in a manner consistent with his high position, and with the respect due to extraordinary talent.

I cannot, however, sacrifice my own convictions to authority; and I feel bound to declare that, in my opinion, no one has ever yet established the identity of the agent in all the phænomena called electric.

Not having the vanity to suppose that the arguments adduced in this Essay will convert a reader in whose mind the hypothesis of identity is already established, I only venture to hope that he may be induced to reconsider the subject, leaving his judgement free to the reception of new impressions. If I succeed so far, my efforts will be sufficiently rewarded.

Far from having exhausted my arguments, I may say with Cicero, "hujus autem orationis difficilius est exitum quam principium, facere: itaque non mihi tam copia quam modus in dicendo quærendus est."

11 Clare Street, Dublin.

XXXII. *On the Decennial Period observed by* Dr. Lamont *in the Magnitude of the Diurnal Motion of the Magnetic Needle. By* P. A. Reslhuber, *Director of the Observatory at Kremsmünster*.*

DURING the past year I was occupied with the reduction and calculation of the magnetic observations which have been made here since the establishment of the magnetic observatory in the year 1839. These observations refer to the absolute determination of the elements of the magnetic force, the daily alteration of the declination and the horizontal intensity, and to the variations of these two elements on the fixed days of observation.

The observations on the change of the declination and horizontal intensity were carried out with the magnetometer of Gauss, being performed daily since the year 1842 at the hour of 8 o'clock A.M., 2 o'clock P.M., and 8 o'clock P.M. mean Göttingen time; I possess therefore already a series of observations extending over a space of ten years, sufficient to permit some inferences to be drawn from the obtained data.

With regard, in the first place, to the decennial period discovered by Dr. Lamont, the following is the result of our observations :—

Magnitude of the Daily Alteration of the Declination from
*8 o'clock A.M. to 2 o'clock P.M.**

	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.	1850.	1851.
Jan. ...	1 58.4	3 9.7	1 9.1	0 49.4	2 9.9	1 9.6	6 26.5	7 51.6	4 12.4	4 59.7
Feb. ...	4 12.1	3 36.9	2 2.9	3 46.2	3 27.5	4 9.6	8 42.2	10 24.2	4 51.5	5 1.1
March.	6 56.6	7 0.9	6 45.4	7 21.5	10 6.0	9 16.9	12 43.6	14 42.0	9 13.9	9 11.0
April...	10 32.4	9 33.7	8 31.5	11 16.0	13 25.7	9 58.7	13 19.7	17 43.2	10 34.1	12 5.4
May ...	....	....	7 43.3	9 54.7	12 45.9	10 20.8	12 49.5	14 17.3	13 54.8	10 12.9
June...	....	9 32.6	7 48.4	9 56.5	12 42.1	11 47.2	13 45.2	14 24.5	13 53.5	13 24.0
July ...	8 24.5	8 55.7	10 11.3	8 50.6	10 52.6	10 39.9	14 34.6	12 16.1	13 32.5	10 50.7
August	7 55.9	8 30.0	9 56.6	9 31.0	10 25.7	12 31.6	14 3.3	11 58.3	11 30.5	8 50.2
Sept...	6 14.6	7 45.8	8 21.2	6 55.6	6 49.8	10 20.8	12 1.2	9 47.5	10 37.9	7 41.0
Oct. ...	6 25.8	5 39.2	7 15.1	6 5.9	6 32.1	11 16.8	12 6.8	7 39.2	9 46.1	7 24.6
Nov....	2 36.1	2 25.8	3 2.2	3 0.5	3 32.5	6 47.4	4 55.5	4 7.1	5 4.4	4 21.2
Dec....	2 13.5	1 58.8	1 59.6	2 37.3	2 23.6	5 8.6	5 27.0	2 32.9	2 41.1	2 2.3
Mean ..	6 33.4	6 28.6	6 14.9	6 39.6	7 56.1	8 42.3	10 55.4	10 39.5	9 8.4	8 0.3

I must remark here in explanation, that these results do not express the *full magnitude* of the daily variation; for, according to my investigations of the hourly change of the declination during the day, the minimum declination occurs with us at 7 o'clock in the morning, Göttingen time, the maximum at 1 o'clock P.M. Göttingen time; hence at 8 o'clock A.M. and at 2 o'clock P.M. the declination had already decreased a little, and therefore the quantities noted are a little too small, but certainly not considerably so; at all events they are sufficient to show clearly the existence of the periodical alteration.

The mean magnitude for the year attains a minimum between 1843–44, a maximum from 1848 to 1849.

I subjected the monthly mean magnitude of the daily change of declination from 1842 to 1850 inclusive to a strict calculation, according to the method used in the case of periodic phænomena, and obtained for the annual course of the above change in the daily declination the following result:—

1842—50.	Observed deviation.	Calculated deviation.
January	3 12.93	2 29.10
February............	5 0.34	5 16.84
March................	9 21.90	9 19.75
April................	11 39.43	11 58.77
May	11 40.91	12 13.97
June	11 43.74	11 16.16
July	10 55.34	10 33.95
August	10 42.60	10 16.91
September	8 47.14	9 33.82
October	8 5.21	7 42.16
November	3 56.84	4 54.49
December	3 0.27	2 32.48
Year	8 10.55	8 10.70

* The quantities wanting in 1842 and 1843 are obtained by interpolation.

The divergence, according to this, is least in the month of January and greatest in the month of May.

It is generally believed that one principal cause of this diurnal change is to be traced to the warming of the earth. I will not venture to decide what part is played by the temperature in the production of these regular alterations; true it is that a minimum of the declination occurs in the morning nearly at the time when the temperature is lowest, and that a maximum occurs in the afternoon about the time when the temperature is highest. But the magnetic declination attains a second minimum between 10 and 12 o'clock at night, and a second maximum between 2 and 4 o'clock in the morning; that is to say, two regular maxima and two minima in the day, while in the temperature only one minimum and one maximum occurs. If the heat were the only or even the chief cause of the magnetic variation, the magnitude of the daily variation of the declination must proceed side by side with the increase and decrease of temperature during the year, which is not proved by the observations which have been thus far made. True it is that the said magnitude is least at the time of lowest temperature; but at the time of highest temperature (July) it has already decreased, having attained its maximum in the month of May.

But another remark involuntarily suggests itself when the foregoing results are reflected upon:—

The magnitude of the daily variation of the declination throughout the months of the year runs parallel with the changes in the humidity of the air; is smallest at the time of greatest humidity, and greatest at the time of maximum dryness.

For the proof of this proposition I give here—

a. The mean diurnal deviation of the declination in the single months.

b. The mean relative atmospheric humidity in parts of 100 in the single months.

c. The mean temperature in the single months from the year 1842 to 1850.

	Mean deviation of the declination from 2ʰ P.M. to 8ʰ A.M.	Mean per-centage of humidity.	Mean temperature in degrees of R.
January	2′ 29″·10	93·84	− 3·08
February.........	5 16·84	91·90	− 0·39
March............	9 19·75	83·77	+ 1·68
April	11 58·77	72·09	+ 6·73
May	12 13·97	70·25	+10·36
June	11 16·16	72·43	+13·19
July	10 33·95	74·37	+14·14
August	10 16·91	76·19	+13·73
September	9 33·82	81·62	+10·50
October	7 42·16	89·07	+ 6·59
November	4 54·49	92·30	+ 2·14
December	2 32·48	94·54	− 0·87
Year	8 10·70	82·69	+ 6·23

The above proposition is proved in a striking manner by the results of the observations of the year 1851, in which an anomaly in the temperature and humidity of the months of May and June is exhibited.

1851.	Mean deviation of the declination from 8ʰ A.M. to 2ʰ P.M.	Mean per-centage of humidity.	Mean temperature in degrees of R.
January	4 59·7	93·74	− 1·49
February.........	5 1·1	91·84	− 1·82
March.............	9 11·0	84·00	+ 2·27
April	12 5·4	73·92	+ 8·13
May	10 12·9	74·28	+ 7·86
June	13 24·0	67·90	+12·89
July	10 50·7	70·74	+13·29
August	8 50·2	73·54	+13·61
September	7 41·0	82·48	+ 9·22
October	7 24·6	84·93	+ 8·23
November	4 21·2	93·60	− 0·72
December	2 2·3	94·56	− 1·21
Year	8 0·3	82·13	+ 5·85

I am unable to explain the connexion of these phænomena, but will leave it to other investigators to follow up this highly interesting fact which has resulted from our observations.

At the end of his paper Dr. Lamont remarks, " that the diurnal deviation of the horizontal intensity is subject to a considerable change; but whether of the same period as that which I have proved to exist in the declination, I am not yet in a position to pronounce, &c."

The investigation of the hourly deviation during a day proves that two maxima and two minima occur, and moreover as follows:—

Between 6ʰ and 7ʰ P.M. mean stand.

Between 10ʰ and 11ʰ P.M. a maximum (the least during the day).

Between 12ʰ and 1ʰ A.M. a minimum (the least during the day, greater than the mean horizontal intensity).

Between 3ʰ and 4ʰ A.M. a maximum (the greatest during the day).

Between 6ʰ and 7ʰ A.M. mean stand.

Between 9ʰ and 11ʰ A.M. a minimum (the greatest during the day, less than the mean horizontal intensity).

The times of the maxima, minima, and mean magnitude of the horizontal intensity occur at earlier hours in the *warm months* than in the *cold ones.*

The horizontal intensity *during the hours of the day* is regularly *smaller,* while during *the hours of the night* it is regularly *greater* than the mean horizontal intensity.

In order to ascertain whether the horizontal intensity is subject to a periodical alteration, I chose from our observations, which had been made three times a day, those made at 8ʰ A.M. and at 8ʰ

P.M. The first falls somewhat before the lowest stand (the greatest minimum), the other a couple of hours before the evening maximum; the difference of both therefore does not give the entire magnitude of the diurnal change in the horizontal intensity.

The deviations, expressed in decimal parts of the absolute horizontal intensity, are as follows:—

Alteration of the Horizontal Intensity from 8 o'clock A.M. to 8 o'clock P.M. Göttingen time. Difference $=8^h$ P.M.$—8^h$ A.M.

	1843.	1844.	1845.	1846.	1847.
January	−0·00048	−0·00076	−0·00098	−0·00011	−0·00066
February.........	− 014	− 038	− 013	+ 064	+ 060
March............	+ 000	+ 090	+ 096	+ 065	+ 109
April	+ 058	+ 158	+ 206	+ 161	+ 347
May	+ 230	+ 245	+ 275	+ 300	+ 412
June	+ 243	+ 402	+ 297	+. 405	+ 355
July	+ 205	+ 279	+ 268	+ 400	+ 385
August	+ 243	+ 314	+ 326	+ 435	+ 418
September	+ 158	+ 292	+ 311	+ 221	+ 325
October	+ 102	+ 117	+ 118	+ 117	+ 050
November	− 010	− 019	− 037	− 004	+ 171
December	− 116	− 112	− 129	− 003	− 131
Year	+0·00088	+0·00138	+0·00133	+0·00180	+0·00208

	1848.	1849.	1850.	1851.	Mean for 9 years.
January	+0·00019	+0·00111	−0·00028	−0·00056	−0·00028
February.........	+ 024	− 008	+ 012	+ 069	+ 024
March............	+ 230	+ 140	+ 095	+ 059	+ 098
April	+ 305	+ 323	+ 220	+. 139	+ 214
May	+ 453	+ 431	+ 437	+ 259	+ 334
June	+ 451	+ 410	+ 395	+ 218	+ 353
July	+ 506	+ 434	+ 341	+ 315	+ 348
August	+ 473	+ 430	+ 372	+ 273	+ 365
September	+ 388	+ 347	+ 335	+ 339	+ 302
October	+ 155	+ 142	+ 171	+ 194	+ 130
November	+ 058	+ 053	+ 033	+ 085	+ 037
December	+ 211	− 047	− 003	− 069	− 044
Year	+0·00273	+0·00230	+0·00203	+0·00152	+0·00178

A minimum of the daily alteration of the horizontal intensity from 8^h A.M. to 8^h P.M. is here plainly shown to fall between 1843 and 1844, and a maximum between 1848 and 1849. Hence the same decennial period occurs in the alteration of the horizontal intensity as has been proved to exist in the diurnal change of declination.

In respect to the course of these alterations during the single months of the year, they appear quite to follow the course of the temperature. Observations made during several years will eliminate the little anomalies which now exist in the warm months, and the law to which all is subject will be thus rendered distinctly visible.

XXXIII. *On the unequal Heating Effect of a Galvanic Current while entering and emerging from a Conductor.* By RICHARD ADIE, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,   Liverpool, July 5, 1852.

IN your Supplementary Number for July, p. 529, I observe a paper by Prof. W. Thomson, wherein he alludes to M. Peltier's experiment on the supposed absorption of heat by a bismuth and antimony joint while conducting a feeble galvanic current passing (on the supposition of there being one current only) from the bismuth to the antimony. In the year 1843, I made some experiments connected with this subject, and as I came to conclusions which gave a different explanation to M. Peltier's researches, I should be glad to avail myself of the medium of your Journal briefly to notice them.

Among the metals bismuth stands as the most imperfect conductor of electricity; and as the heat developed by an electrical current passing along a bar is in proportion to the resistance to conduction, it follows that, in compound metallic bars of uniform sectional area throughout, the bismuth portion is more heated than the other parts. In a compound bar composed of antimony, bismuth and antimony, again, when a feeble galvanic current is made to pass through this arrangement, the conditions required for observing M. Peltier's experiment are supplied; for at the one end of the bismuth bar the current through the joint is from antimony to bismuth, while at the other end of the bismuth bar the passage through the joint is from bismuth to antimony. To ascertain the temperatures at these joints, two delicate thermometers are attached and enveloped with a little dry cotton wool. A galvanic current made to pass through this arrangement elevates the thermometer where the electricity enters the bismuth more than the one where it quits the bismuth; the difference between these two thermometers varying with the changes in the battery from which the electrical current is derived. Now, according to Peltier's views, the thermometer at the joint where the current passes from the bismuth to the antimony, should, during the conduction of a feeble current, stand lower than the temperature of the atmosphere; to ascertain this point I have frequently repeated the experiments without being able to note any absorption of heat. With me the thermometer stands a little above the temperature of the atmosphere; and from a brief notice I have seen of M. Becquerel's investigation of this question, I believe that he has likewise failed to note any absorption of heat. The two joints of the bismuth

invariably showed a different temperature, the one where the electrical current quitted the bismuth to enter the antimony being the lower, but always above the temperature of the surrounding air. This property found in the bismuth bar, which was directly derived from the examination of Peltier's law, extends to any other kind of matter, whether in the gaseous, fluid, or solid state, traversed by a galvanic current; the only condition required for the experiment being that the current be brought to the body tested by conductors which offer less resistance to its passage than the body itself *.

Thus, although my experiments gave a different explanation of the observations of M. Peltier, they did not lessen their value; for through the study of this supposed absorption of heat during the passage of a feeble galvanic current a yet more general law was established, by which a consistent explanation was given to a remarkable fact noted in Daniell's Introduction to Chemical Philosophy, considered by the author of that work at the time of its publication not to admit of illustration. The fact in question is one of the conducting wires from a powerful battery becoming red-hot, while the other wire remains comparatively cool; in this case the galvanic current has to pass across a short space of air, where it encounters great resistance, and thus heats the wire, where it meets this resisting force much more than the wire where it quits the obstructing medium; the result being the same as those exhibited in the case of the bismuth bar cited above, the difference being only one of degree.

In studying the effects presented to us in thermo-electrical arrangements, the force designated by the term attraction of cohesion is constantly found to form at the present time an impenetrable barrier; the various properties of metals in their relations to the imponderables, to tenacity, elasticity, &c., show what modifications the force of cohesion of attraction can undergo; yet in the present state of science there are no means of ascertaining what alteration in this force has taken place, save through the final effect shown by changes in the metal or its alloys. Any one who may reflect on the subject will perceive what an extensive range of phænomena is here hidden from view; and they will probably think that there the laws, which at present cannot be traced so as to connect the generation of electricity by chemical action with that derived from thermal sources, may ultimately be developed.

Yours very respectfully,
RICHARD ADIE.

* Edinb. New Phil. Magazine, vol. xxxvii. p. 301.

XXXIV. *Notices respecting New Books.*

On Animal Electricity; being an Abstract of the Discoveries of Emil
du Bois-Reymond, *Member of the Academy of Sciences, Berlin, &c.
Edited by* H. Bence Jones, *M.D., F.R.S., Physician to St. George's
Hospital.* London: John Churchill, Princes Street, Soho.

WE have long wished to see an English abstract of the researches
of M. du Bois-Reymond, and the book before us answers to
the wish. It commences with a brief but extremely interesting historic
introduction, in which, as might be expected, Galvani and Volta
are the principal figures. Comparing these two celebrated men, the
author observes,—

" No one who wishes to judge impartially of the scientific history
of these times and of its leaders, will consider Galvani and Volta as
equals, or deny the vast superiority of the latter over all his oppo-
nents or fellow-workers, more especially over those of the Bologna
school. We shall scarcely again find in one man gifts so rich and so
calculated for research as were combined in Volta. He possessed
that ' incomprehensible talent,' as Dove has called it, for separating
the essential from the immaterial in complicated phænomena ; that
boldness of invention which must precede experiment, controlled by
the most strict and cautious mode of manipulation ; that unremitting
attention which allows no circumstance to pass unnoticed ; lastly,
with so much acuteness, so much simplicity, so much grandeur of
conception, combined with such depth of thought, he had a hand
which was the hand of a workman."

The progress of discovery in this department of science is sketched,
and the author afterwards passes on to describe his instruments and
manner of experiment. We have a valuable and instructive chapter
on the improved galvanometer. The helix of the larger instrument
used by the author consists of the astonishing length of 3·17 English
miles of copper wire in 24,160 coils ! It would be difficult, if not im-
possible, without drawings to give an intelligible description of the
author's mode of experiment. Every precaution which experience
could suggest, and the most refined manual dexterity could apply,
has been taken to secure accuracy, and rescue the results from inci-
dental disturbances. The main feature in the experiments is, that
the contact of metals with muscle or nerve, or with each other, is as
much as possible avoided, connexion being established by cushions
of bibulous paper moistened with a saturated solution of salt and
water. Nor is contact with even these permitted, lest an irritating
action should be exerted upon the tissue ; the cushions are protected
by a cover of pig's bladder with a little albumen spread over it, and
upon or against this the tissue to be examined is laid.

The great law established by the author, and of which the so-called
frog current, together with the various phænomena observed by
M. Matteucci, are to be regarded as particular manifestations, more
or less complicated, is one of extreme simplicity. Let us suppose
the circuit all complete with the exception of one small gap, at each
side of which stands a cushion of bibulous paper moistened and pro-

tected in the manner already described. Let this gap be closed by the introduction of a cylindrical or prismatic piece of muscle, one of the transverse sections of which is caused to abut against one of the cushions, and the other transverse section against the opposite cushion. The circuit is now complete, the muscle playing the part of a little battery; the galvanometer is included in the circuit; and if a current be produced, it will exhibit itself at the galvanometer. In the above arrangement, however, *no current is produced.* Let one of the ends of the muscle remain in contact with the cushion, as before, and let the muscle be bent up and caused to rest with its longitudinal section against the cushion; a strong deflection is the immediate consequence. The direction of this muscular current is always from the transverse section through the galvanometer wire to the longitudinal section; and hence the law of action is, that *every point in the longitudinal section is positive to every point in the transverse section.* The current varies in intensity when the points of the muscle which come into contact with the cushions are changed, and the points of maximum and minimum action are determined in a most delicate manner. The distinction between upward and downward currents the author regards as non-essential; in fact, it altogether depends upon which end of the muscle is in contact with the cushion whether the current is up or down. The part played by the tendon is that of a passive conductor of the current generated in the muscle itself.

The author's experiments clear up the doubt which existed regarding the influence of contraction on the muscular current. He proves that, in the act of contraction, the muscular current is always diminished. A single contraction is unable to show any effect upon the needle, on account of the inertia of the latter; but when a continuous spasmodic action is kept up, the effect becomes evident. The convulsions may be obtained by gradually destroying the motor nerve, by a chemical agent, by poisoning the animal with strychnine, by passing a current of electricity of varying intensity through the nerve, or by submitting the latter, by means of a break-circuit wheel, to a series of successive shocks. From these experiments we select the following one, as it is explanatory of another which has caused some discussion among men of science. A live frog was taken and one of its feet was dipped into a vessel containing a conducting liquid, while the other foot was dipped into a second vessel of the same kind; the ends of the galvanometer wires were also connected with the vessels. One leg was paralysed by having its ischiatic plexus cut through. The animal was then poisoned by strychnine, and convulsions were the consequence. Now in one leg these convulsions diminished the muscular current, whereas in the passive leg no such diminution took place; the equilibrium was therefore destroyed, and a current exhibited itself on the galvanometer.

It was this result that suggested the celebrated experiment which gave rise to the discussion before alluded to,—a discussion in which the veteran Humboldt took the leading affirmative position. Du Bois-Reymond removed the frog's feet from the fluid and put his own fingers in their place; one arm he left passive like the paralysed leg

of the frog, while he strongly contracted the muscles of the other arm. The expected result at once exhibited itself, and a considerable deflection was obtained. This result has been abundantly corroborated; the writer may perhaps be permitted to contribute his personal testimony, he having on a first trial obtained a deflection of thirty degrees. The sense of the deflection depends upon the arm contracted; on changing the arm, the deflection is in the opposite sense.

The electric deportment of the nerves has often been a subject of anxious inquiry. A nerve possesses a current which exhibits itself in a manner precisely similar to the muscular one. The arrangement of an experiment with the nerve is in substance the same as that applied to a muscle, and the direction of the current follows the same law. It proceeds from the transverse section through the connecting wire to the longitudinal section. It is an error to suppose that the various tissues of the animal body are electromotive towards each other. If the current due to each particular be shut out, Du Bois-Reymond shows that no possible combination of muscle, nerve, tendon, skin and bone, can produce any electric action.

Some slight alterations will probably suggest themselves to the translator in the preparation of a second edition. The name of the author—not an easy one to English organs—occurs too often; and the polemical tone of the book might, in certain places, be softened down with advantage. In a work of such intrinsic value no such seasoning is required. The letters referring to the diagram at page 33 need a trifling correction; and in one or two cases the word 'observation,' although the correct equivalent of the German *Beobachtung,* might be changed for some other word which would not at the same time answer to *Bemerkung.* On the whole, however, the translation has been carried out with care and fidelity; and the English investigator must feel indebted to Dr. Bence Jones for placing such a valuable work within his reach.

Atmosphere: a Philosophical Work. By GEORGE WOODHEAD, *Esq.* London: Hippolyte Baillière.

This book consists of a number of articles, communicated from time to time to the Mechanics' Magazine by the author, and doubtless thought too valuable by him to be permitted to slumber in obscurity. The avowed design of the work is to explain the causes of certain effects whose hidden springs have eluded the researches of all philosophers, those of Greece included, up to the time when nature, forgetful of her previous anguish, rejoiced over the advent of a Woodhead.

With regard to the work, it is our duty to state that we have rarely seen so much nonsense crammed into so small a space. We do not blame Mr. Woodhead for this—not at all; the matter is evidently due to circumstances beyond his control. There is a moral Daltonism in the world as well as a physical one; and as reasonably might we censure the great discoverer of the atomic theory for his devotion to one or two colours, as Mr. Woodhead for his adherence to two ideas—*he* can't help it; the blame is not his but another's.

Light and air are regarded by our author as the two great powers which uphold the universe. In his opening paragraph he naively inquires, " Is not the agitation of boiling water caused by air which enters through the bottom of the vessel, and which rising up through the water causes the bubbling called boiling ? " We really imagined that Mr. Woodhead, in stating the case thus, had condescendingly placed himself in the position of some interesting little prattler, standing at his nurse's knee, and putting to the said nurse the above philosophical query ; and that Mr. Woodhead, in his own benign way, was going to set the little questioner right. But no— this is Mr. Woodhead's own opinion. He believes that the air actually enters in the manner described ; he believes that light is air in a state of radiation ; and his theory of caloric is, that light penetrates bodies and makes way for the admission of air into them ; and it is the expressed air of a red-hot piece of iron, which, when it is immersed in water, causes the ebullition, repulsion, expansion, steam and hissing.

It is sometimes mournful to observe how the inventions of one age render the pains and labours of the preceding one valueless. The ponderous aqueducts of ancient Rome are rendered useless by the application of the simplest laws of hydraulics ; canals are superseded by railways ; and the Manchester cotton-spinner has often to cast sound and costly machinery aside, to avail himself of some new invention. Philosophic endeavour is doomed, in the eternal progression of things, to share a similar fate ; and it is with a certain exalted sorrow that we contemplate the efforts of Mr. Woodhead's predecessors. Upwards of thirty years ago Colonel Sabine took up his cold and perilous post on Melville Island to make pendulum experiments, and deemed himself lucky in getting away from the place, after a winter's exile, without any accident save the loss of five frost-bitten fingers by an incautious artilleryman. But his troubles might have been spared had our latter-day genius been present to tell him that he was pursuing a phantom, and beating the air, in another sense than the mere literal one, when he set his pendulums agoing. The retardation of the pendulum and the decrease in the intensity of gravity in the equatoreal regions, quoth Mr. Woodhead, are attributable to the increased density of the atmosphere in these regions. His theory is, that the centrifugal force arising from the earth's diurnal rotation causes an accumulation of air, and a consequent increase of pressure at the equator,—not at all regarding the mathematical fact, that were the motion of the earth seventeen times quicker than at present, poor Mr. Woodhead himself, if placed at the equator, would dance upon nothing, and exercise no practical pressure at all.

But, to Mr. Woodhead's mind, the dip of the magnetic needle affords still stronger and more striking evidence of the above arrangement of the atmosphere. In the direction of the poles the atmospheric resistance is least ; the needle points in the direction of least resistance, and hence its polarity. What will Carl Friedrich Gauss say to this ? Here is also a morsel for the geologist :—The increase of heat observed as we descend a shaft is caused by the superincum-

bent pressure of the column of air. And for the physiologist :—
Animal heat appears to be caused by the pressure of air in the lungs.
And for the botanist :—The ascent of sap, and the general upward
tendency of trees and plants, are due to atmospheric pressure, which
is greatest at the roots ; the trees and sap ascend, as a balloon in
air, or as a piece of wood in water !

"It is generally thought that in electrolytic decomposition the
gases called oxygen and hydrogen are somehow formed from the
water ; " but this is not at all our author's opinion. He thinks they
are derived from the atmosphere, and that they come from the bat-
tery through the conducting wires ; and further, that it is the con-
fluence and pressure of these two aëriform fluids which produce the
electric light, the incandescence of wires, and the other calorific and
luminous phænomena of the circuit.

"Light," says Mr. Woodhead, "can be caught and examined."
There is no salt used in the process, and herein Mr. Woodhead's
experiments differ from certain of our own made on sparrows and
cockrobins at the beginning of the present century. He 'catches'
the light by ingeniously entrapping it in sealing-wax. Again, solar
light is the power which moves the earth, both on its axis and in its
orbit, and seems also to regulate the motions of the planets. More
solar light appears to impinge upon the earth at the solstices than at
the equinoxes, which Mr. Woodhead sagaciously supposes may ac-
count for the variation of the earth's distance from the sun. Sir
John Herschel will no doubt be interested to learn all this ; he will
henceforth be able to assign their proper value—or no-value—to the
experiments of Mr. Bennet, while a new life begins to palpitate
under the ribs of the defunct corpuscular theory !

It is Leigh Hunt, if we remember aright, who insists on the ne-
cessity of *contrast* in the composition of what is called humour ; such
a contrast, we imagine, exists when we behold a man talking extreme
nonsense with a grave face ; and as a choice specimen in this line,
we can conscientiously refer to the book of Mr. Woodhead.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 29, "FURTHER Experiments on Light." By Henry Lord
1852.   Brougham, F.R.S., Member of the Institute of France,
and of the Royal Academy of Sciences of Naples.

The author commences this account of his experiments by remark-
ing, that " it is probable that some may consider the inference to be
drawn from the following experiments as unfavourable to the doc-
trines of my former paper—I think I can explain the phenomena
according to those doctrines—but be they ever so repugnant, we are
of course in search of truth, and have no right even to wish that the
balance may incline one way rather than another, far less to conceal
any facts which may affect its inclination."

The leading experiment is this :—A speculum is placed in a beam

of light and is inclined so that the reflected rays shall make a small angle with the surfaces. Near the speculum the axis of reflected rays coincides with that of the direct rays, but at a greater distance the two discs are separate. The speculum being placed horizontally across the pencil, coloured fringes appear both on the upper and lower side of the reflected disc. These two sets of fringes are alike in their colours and in the order of their colours, but the upper fringes are narrower than the lower, and they diminish in breadth with their distance from the disc, while the lower ones increase in breadth with their distance. If only one edge of the speculum is in the pencil there are only fringes on one side of the disc.

It appears that the breadth of the fringes is in some inverse proportion to the breadth of the speculum. When the speculum is a triangle with a very acute angle, the broadest fringes, and those most removed from the disc, answer to the points of the speculum where it is narrowest, and they increase regularly towards the point which answers to the acute angle or apex of the speculum. Their form is hyperbolic.

When the edges of the speculum are parallel, the disc near to it is filled with groups of fringes which vary in number, in breadth and in colour, at all the distances from the speculum. At one distance they form only a dark line running through the disc, and this is deep purple when examined closely. At a greater distance the fringes have other colours, and become broader again; and at a still greater distance they emerge into the shadow on both sides of the disc.

The phenomena of reflexion, it is stated, closely resemble those of flexion, as to the fringes, their colours, their magnitude, their varia- tion at different distances from the bending edges, and at different distances of those edges from each other.

A convenient method of examining the variation of the fringes, whether of reflexion or of flexion, at various distances, is to incline the screen upon which they are received, so that it crosses the rays forming the fringes, which are exhibited upon it, at various distances from the edges. The line which each fringe describes being the projection of the line which the rays follow that form the fringe, we can in this manner observe if the course of these rays after flexion is rectilinear or curvilinear, the projection being, generally speaking, a line of the same kind with the original line; and at least never rec- tilinear if that original line is curvilinear.

If $y = f(x)$ be the line which the rays follow after flexion; ϕ the angle of the screen's inclination; $\dfrac{\sin \phi}{\cos \phi} = m$; and x^1 the abscissæ of the line of projection; then its equation is $y = f(\sqrt{1 + m^2} \cdot x^1)$. If the curve of the rays be supposed to be the equilateral conic hyper- bola, the radius of curvature in the curve of projection, it is stated, must be less than that in the original line; and so the curvature is more easily discerned by the eye. As under no circumstances of inclination of the screen, and at no part whatever of the course of the fringes could the author perceive the least difference of form from all the other parts, he infers, either that the rays follow a rectilinear course, or that their deviation from it must be very small.

Though the phenomenon seem to indicate a crossing of the rays both in flexion and reflexion, at or near the distance at which the dark or deep purple line is formed, yet the author has never been able to observe that an obstacle placed between that point and the speculum (or the bending edges), made the fringes on the opposite side of the disc at the screen to disappear, but only the fringes on the same side with itself.

Referring to Fresnel's memoir, the author states that the principle laid down in it, " that the dilatation of the fringes depends solely upon the breadth of the aperture," will not afford an explanation of the phenomena described in his former paper respecting fringes formed by edges acting in succession, for he there showed that their breadth and their distances from the direct rays are in the inverse proportion of the distance of the edges; and if the edges are so placed that the rays pass parallel to each other, and not diverging, and the edges are moved to different distances in the same line, *e. g.* horizontally, then their distance from each other vertically being the same, the aperture is the same at all distances of the edges from each other horizontally, and yet the breadth of the fringes is inversely as the horizontal distance. Further, where the edges are not placed in succession, but directly opposite to each other, the breadths of the fringes do not appear to follow the exact inverse proportion of the distances of the edges (that is the size of the aperture), the observed breadths corresponding more nearly with the curve $y = \dfrac{m}{x} + \dfrac{m}{x^2}$, x being the distance of the edges, and y the breadth of the fringes.

The author considers that the internal fringes, or those of the shadows of small bodies, called fringes of interference, require a more full examination than they have received in certain respects. As regards the central space and the two deep black fringes or intervals on each side of it, he remarks that no examination with a magnifier, and no inclination of the screen, at all resolves these colours into purple as in the dark line before described. They appear to follow a different law from that of the coloured ones as regards their breadths in proportion to their distances from the pin or other small object, at least if they are caused by interference, and if the effect of interference is inversely as the difference of the length of the rays; for that would give for the breadths the curve $y = \dfrac{m}{\sqrt{a^2 + x^2} - \sqrt{b^2 + x^2}}$, which nowise agrees with the admeasurements.

The action of transparent plates on the rays, in bending them, resembles in every respect that of opake plates, except that there being no shadow, the external fringes are not perceived. But the shadow of the edge of the plate is surrounded by two sets of fringes resembling exactly those surrounding the shadow of a hair or other small body placed upon the plate's edge, and following its course, with this only difference, that this shadow of the transparent plate's edge has no internal fringes as the hair or other small body's shadow has.

May 6.—A paper was read, entitled, " On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances."— No. II. By Colonel Edward Sabine, R.A., Treas. and V.P.R.S. &c.

From the discussion of the magnetic observations made at Toronto and Hobarton in the years 1843, 44, 45, the author in a former paper adduced evidence of the existence of periodical laws by which the principal disturbances of the magnetic declination appeared to be regulated. Having since had occasion to examine the disturbances of the Declination at the same two stations in the three succeeding years 1846, 47, 48, he states that he had the satisfaction of finding that the observations of these years confirm every deduction which he had ventured to make from the analysis of the disturbances of the former period ; whilst new and important features have presented themselves in the comparison of the frequency and amount of the disturbances in *different years*, apparently indicating the existence of a *periodical variation*, which, either from a real or causal connection, or by a singular coincidence, corresponds precisely, both in period and epoch, with the variation in the frequency and magnitude of the solar spots, recently announced by M. Schwabe as the result of his systematic and long-continued observations.

The method pursued in examining the laws of the Declination-disturbances in 1846, 47, 48, is the same as that adopted in the three preceding years. Every hourly observation which was found to differ a certain amount from the mean value of the Declination in the same month and at the same hour was, as before, separated from the rest. The number of observations thus separated in the period commencing July 1, 1843, and ending July 1, 1848, was at Toronto 3940, and at Hobarton 3469, being respectively 1 in 9·43 at Toronto, and 1 in 10·55 at Hobarton, of the whole number of hourly observations. The disturbed observations being distributed into the several hours, months, and years in which they had occurred, their numbers and aggregate values in each particular hour, month, and year, were ascertained. They were then divided into easterly and westerly deflections, and the same process of distribution was gone through with each of the divisions. The *mean* hourly, monthly and yearly number and aggregate values in the whole period were then taken as the respective units, and the ratios to these units computed for each of the hours, months and years ; whereby the relations, whether of numbers or of aggregate values in different hours, different months, and different years, were shown.

The results thus obtained are discussed separately in the following order :—

I. Inequality or variation in the number and aggregate values of the disturbed observations in *different hours*. This examination is made by classing together—1st, easterly disturbances at Toronto and westerly at Hobarton ; and 2nd, westerly at Toronto and easterly at Hobarton.

From the first classification, it appears that at both stations there are fewer disturbances, and their aggregate values are less in the hours of the day than in those of the night ; that 9 P.M. is the hour of the maximum of frequency and also of value at Toronto, and 11 P.M. at Hobarton ; and that the periods of minima are between 2 and 3 P.M. at Toronto, and between 5 and 6 A.M. at Hobarton.

It appears further that the average value has a similar law of variation to that of the number and aggregate value.

The second classification shows that at Hobarton the contrast both in frequency and aggregate value is still between the hours of the day and those of the night, the ratios being, however, in this case greater than unity during the former hours, and less than unity during the latter, contrary to what takes place with the easterly disturbances : at Toronto the contrast is between the hours from noon to midnight, and those from midnight to noon, the ratios being greater than unity during the latter hours, and less than unity during the former. In both cases the variation in the ratios appears to be dependent on the hours of *local*, not on those of absolute time.

From a table showing the ratios of easterly aggregate values to westerly at Toronto, and of westerly to easterly at Hobarton, it appears that, at both stations, the deflection (due to disturbance) of the end of the magnet of the same name as the magnetic latitude is to the west during the hours of the day or from 5 A.M. to 5 P.M. : at a little before 6 P.M. at Toronto, and a little after 6 at Hobarton, the deflections pass through zero (or the undisturbed position of the magnet) into easterly deflections of that end. The magnitude of those deflections rapidly augments to a maximum at 9 P.M. at Toronto, and at 10 P.M. at Hobarton ; they again pass through zero between 4 and 5 A.M. ; and attain the westerly maximum at 7 A.M., the variation in the magnetic direction due to the disturbances depending, like those of number and value, on the hour of local time.

II. Inequality or variation in the number and aggregate values of the disturbed observations in *different months*. From the tables which are given, it is obvious that there is a systematic variation in the numbers and aggregate values of the disturbances in the different months ; and at both stations the easterly and westerly ratios, separately considered, differ little in the characters which they assign to the variation, from the ratios of the two combined. The most distinctly marked feature is that the disturbances are less frequent and have a less aggregate value in November to February at Toronto, and in May to August at Hobarton, than in the other months respectively : so that the disturbances are governed by a law depending either on the period of the year, or on *local* season, not on absolute time.

III. Variation in the number and aggregate values of the disturbed observations in *different years*. Taking the ratios of the numbers and aggregate values of the disturbed observations at Toronto and Hobarton in the different years (from 1843 to 1848), to the average annual number and aggregate value respectively, it appears that there is a remarkable correspondence in the variation of these ratios in different years at the two stations ; and that at each, both ratios increase progressively from 1843 to 1848, with the single exception of 1845, in which there is a small diminution in that of the number and also that of the value. Taking the mean of the ratios at Toronto and Hobarton, the ratio of the number increases

from 0·60 in 1843 to 1·43 in 1848, and the ratio of the value from 0·52 in 1843 to 1·51 in 1848, the variation in each having much more the aspect of a *periodical inequality* than of an accidental variation. Looking to the theoretical importance of the existence of a periodical inequality of this nature, affecting at the same time, and in the same manner, parts of the globe most remote from each other, the author refers to the confirmation it may obtain from contemporaneous observations at other stations. Pending such confirmations he remarks that this progressive increase in the amount of disturbance at Toronto and Hobarton, between the years 1843 and 1848, derives great additional interest and importance from its apparent connection with an equally remarkable progressive increase which took place at the same two stations, in the magnitude of the diurnal range of the Declination in the same years. From the mean magnitude of the diurnal variation of the Declination in each month, tables are deduced showing the mean magnitude or ranges in the four months constituting the respective seasons, and in the twelve months constituting the year, in each year from 1843 to 1848, both at Toronto and at Hobarton. From these tables it appears that at each station, for each of the seasons and for the whole year, the diurnal range of the Declination had a progressive increase during that period ; the increase for the whole year being from 8′·90 in 1843 to 12′·04 in 1848 at Toronto, and from 7′·66 to 11′·43 at Hobarton. In support of the opinion that these progressive increases in the range of the diurnal variation at two stations separated from each other by nearly half the surface of the globe are independent and corresponding measures of a general phenomenon, the author adduces the results obtained by Dr. Lamont from the observations at Munich. From these it appears that the mean range of the diurnal variation in monthly periods at Munich increased progressively from 7·82 in 1843 to 11·15 in 1848.

The author remarks that the increase so distinctly marked in the two classes of phenomena between the years 1843 and 1848 tends to indicate a causal connection subsisting between the disturbances and the regular diurnal variation. If we suppose the diurnal variation to be divided into two portions, one of which is nearly uniform in amount throughout the year (at the same station), whilst the other has a hemispherical phase, developed in either hemisphere according as the sun is in the northern or the southern signs,—it is the former of these two portions which sustains the variation consistent with and apparently related to the variation in the number and values of the disturbances.

That the progressive increase in the mean monthly diurnal range, from 1843 to 1848, was not confined at Toronto and Hobarton to the Declination only, but took place likewise in the diurnal variations of the Inclination and Total Force, is shown by the tables which are given.

In conclusion the author observes, that " in our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical con-

nection which may throw light on a subject as yet so obscure should not be altogether overlooked. As the sun must be recognised as at least the *primary* source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should also look in the first instance to any periodical variation by which we may learn that the sun is affected, to see whether any coincidence of period or epoch is traceable. Now the facts of the solar spots, as they have been recently made known to us by the assiduous and systematic labours of Schwabe, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope, or photosphere, of the sun; and it is certainly a most striking coincidence that the period, and the epochs of maxima and minima, which M. Schwabe has assigned to the variation of the solar spots, are absolutely identical with those which have been here assigned to the magnetic variations." From the results of his observations of the solar spots from the years 1826 to 1850, M. Schwabe has derived the conclusion that " the numbers in the table leave no room to doubt that, at least from the years 1826 to 1850, the solar spots have shown a period of about ten years, with maxima in 1828, 1837, and 1848, and minima in 1833 and 1843." M. Schwabe has not been able to derive from the indications of the thermometer or barometer any sensible connection between climatic conditions and the number of spots. The same remark would of course hold good in respect to the connection of climatic conditions with the magnetic inequalities, as their periodical variation corresponds with that of the solar spots. But it is quite conceivable that affections of the gaseous envelope of the sun, or the causes occasioning those affections, may give rise to sensible magnetical effects at the surface of our planet, without producing sensible thermic effects.

XXXVI. *Intelligence and Miscellaneous Articles.*

ON A BRILLIANT METEOR SEEN AT SIDMOUTH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,   Sidmouth, August 23, 1852.

I HAVE been favoured by two ladies with the subjoined account of a splendid meteor which they saw on the night of the 12th of the present month. Probably the whole or a part of their letters may be deemed worthy of insertion in your Journal. I should premise that the ladies were in company together when they saw the meteor—that their accounts of it were afterwards written independently of each other—that the time mentioned may be considered within a minute or two of the mean time at Sidmouth—that the degrees named were *estimated* only, as was also the bearing of the meteor. Several other persons here saw the phænomenon, but I have been unable to obtain any *accurate* account of time, altitude or bear-

ings. A rough trial with a common quadrant the following day gave, from the position indicated by a sailor who saw the meteor (probably at its greatest altitude), about 30°, and about 10° E. of magnetic north. It is described by *all* as having cast a strong shadow—that the hour could have been seen on a watch—that while globular it approached the full moon in apparent size—that no noise was heard during its appearance. I also subjoin an extract from Woolmer's Exeter and Plymouth Gazette, which evidently refers to the same meteor.

I am, Gentlemen,

Respectfully yours,

N. S. Heineken.

Letter No. 1.

"Sidmouth, August 13, 1852.

"I have written down, according to your request, all the particulars I can remember respecting the beautiful meteor which I saw last evening. It was about 12 minutes past 9 P.M. when, as I was walking home, a light—so bright that my figure cast a strong shadow across it—streamed upon the road. I instantly turned round and saw in the sky, about 15° W. of Cassiopeia, a meteor the size of the full moon, and of a warm yellow colour. The ball immediately shot out into a bar, apparently 7° 30′ in length and 1° wide. The edges of the bar were sharply defined, and the breadth was the same throughout; but both the ends were jagged, and in the centre there was a rent which gradually became wider, until within 30 seconds, as near as I could guess the time, there was a distance of 30′ between the two portions. In about 30 seconds more the bar lost its sharply defined appearance and faded into a thin luminous cloud with a pale diffused light, which disappeared altogether in the course of two minutes. The meteor appeared to me to be stationary after it had shot out into the bar, until it had faded into the luminous cloud, which I fancied had a slow motion towards the east.

"Anne R. Bennett."

Letter No. 2.

"Sidmouth, August 13, 1852.

"As I was returning home about 12 minutes past 9 P.M. on Thursday the 12th instant, I was startled by seeing the road suddenly illuminated by some brilliant light behind me. Turning instantly I saw a bright body in the sky, due north, I think, half-way between the pole star and the horizon, the constellation Cassiopeia being to the east of it. The meteor rapidly spread into a horizontal bar about 7½ degrees long and scarcely one in width, the centre and extremities of a beautiful pale green—two points *between* bright yellow shading into the green on either side; the centre became paler, as though a separation were taking place, and gradually the meteor lost its brilliancy and defined form, becoming more like a small luminous cloud which slowly faded from my sight. The meteor appeared stationary, and was visible for 2 minutes, but the intense light lasted scarcely 30 seconds.

"H. N. Smith."

Extract from Woolmer's Gazette.

" On the evening of the 12th of August, at ½ past 9 (query London time), a meteor broke forth with a slight report, as from the nipple of a percussion-gun, illuminating the atmosphere around, at an elevation of 48° N.E. midway between Perseus and Cassiopeia, and remained stationary and luminous for a few minutes, radiating with its point to the east.

" JOHN BREMRIDGE."

" Southmolton, Aug. 16."

P.S. I may also add, that on the Monday night previous at nearly eleven o'clock, a meteor, equally if not more splendid, passed over the town of Sidmouth from north to south, casting a light by which the hour upon a watch might have been seen; but of this meteor I have been unable to obtain any further particulars.

N. S. H.

ON THE INDIRECT BLEACHING POWER OF MERCURY.
BY C. F. SCHÖNBEIN.

I have long since shown that mercury possesses the power of communicating to oxygen that condition in virtue of which it colours guaiacum tincture blue, decomposes iodide of zinc, &c., and produces those general oxidating effects which are caused by ozone. The fact that the latter destroys organic colouring matters, suggested the idea that oxygen under the influence of mercury would likewise effect this change, and the following experiments prove that this is really the case.

When 200 grms. of mercury and 10 grms. of water, sensibly coloured with indigo-solution or an alkaline indigo-sulphate, are shaken briskly for some time in a tolerably capacious flask containing oxygen or atmospheric air, it is decolorized precisely as if it had been treated with ozone, chlorine or oxygenized turpentine, &c. Elevation of temperature quickens this decolorization. Water coloured by cochineal or logwood may be decolorized in a similar manner, whence it may be inferred that oxygen in contact with mercury is capable of destroying all organic blue and red colours. I have recently fully described the decolorization of indigo solution by oxygen in contact with phosphorus; it may therefore be said that mercury acts upon vegetable colours like phosphorus, though in a much weaker degree, that is to say, both bodies, like so many other inorganic and organic bodies, possess an indirect power of bleaching. If platinum, gold and silver were volatile at ordinary temperatures, they would also destroy organic colouring matters when shaken with their aqueous solutions and oxygen. Some years ago I showed that moistened paper coloured with indigo-solution was bleached in 24 hours by contact with spongy platinum.—*Journ. für Prakt. Chem.,* lvi. p. 353.

ON THE INDIRECT BLEACHING POWER OF STIBÆTHYLE.

During the last year Prof. Löwig and myself made some experiments in the laboratory at Zurich upon stibæthyle, in order to test its power of bleaching, and it turned out that this remarkable body destroyed the colour of indigo-solutions still more energetically than even phosphorus. We added a small quantity of stibæthyle to a comparatively large amount of indigo-solution, shook the whole with atmospheric air, and found that the colour was destroyed in a few seconds.

There can be no doubt that stibmethyle, kakodyle and similar compounds would act like the stibæthyle. These substances are so oxidizable that they take fire in atmospheric air even, at the ordinary temperature ; and it may be inferred that they are more powerful exciters of oxygen than phosphorus, and consequently possess a great power of bleaching.—*Ibid.*

METEOROLOGICAL OBSERVATIONS FOR JULY 1852.

Chiswick.—July 1. Fine : cloudy : slightly overcast. 2. Cloudy and fine. 3, 4. Very fine. 5. Excessively hot : thermometer higher in the shade than it has been for at least twenty-six years : lightning at night. 6. Very hot. 7. Cloudless : hot and dry. 8. Dry haze : sultry : clear at night. 9. Very hot. 10. Very fine. 11. Hot and clear. 12. Sultry. 13. Fine : lightning, with distant thunder at night. 14. Overcast : thunder : very hot : lightning, with rain at night. 15. Cloudy and fine : clear. 16. Slight haze : very hot : excessively heavy and constant rain at night. 17. Rain : cloudy and warm : clear at night. 18. Very fine : heavy clouds : clear. 19. Very fine. 20. Overcast. 21. Light clouds : very fine : clear. 22—24. Very fine. 25. Overcast : thunder : rain. 26. Cloudy and fine : clear. 27. Slight haze : very fine. 28—30. Very fine. 31. Heavy dew : very fine : cloudy.

Mean temperature of the month	67°·37
Mean temperature of July 1851	60 ·71
Mean temperature of July for the last twenty-six years	63 ·40
Average amount of rain in July	2·37 inches.

Boston.—July 1, 2. Fine. 3. Cloudy. 4. Fine : thermometer 84° at 5 P.M. 5. Fine : therm. 91° at 2 P.M. 6. Fine : therm. 86° at 3 F.M. 7. Fine : therm. 81° at 3 P.M. 8. Fine. 9. Fine : therm. 89° at 2 P.M. 10, 11. Fine. 12. Cloudy. 13. Fine. 14. Cloudy. 15. Cloudy : rain, with thunder and lightning early A.M. 16. Fine : rain, with thunder and lightning P.M. : therm. 86°. 17. Cloudy : therm. 86° 3 P.M. 18. Fine. 19—22. Cloudy. 23. Fine. 24. Cloudy. 25. Fine : rain P.M. 26. Cloudy : rain A.M. and P.M. 27, 28. Fine. 29. Cloudy. 30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—July 1. Bright : cloudy. 2. Rain : cloudy. 3. Bright : cloudy : clear. 4. Cloudy : clear : fine. 5. Bright : clear : cloudy : thunder and lightning. 6. Rain : cloudy : fine. 7. Hazy : fine. 8. Bright : fine : fog. 9. Hazy : showers : thunder and lightning. 10. Bright : cloudy. 11. Bright : clear : fine. 12. Bright : fine : cloudy : fine. 13, 14. Bright : fine : clear : fine. 15. Bright : fine : cloudy : fine. 16. Hazy : fine : clear : fine. 17. Cloudy : rain. 18. Bright : cloudy : clear : fine. 19. Hazy : cloudy : clear : fine. 20. Bright : cloudy : rain : fine. 21. Rain : cloudy : fine. 22. Bright : hazy : fine. 23. Bright : fine : cloudy : fine. 24. Drops : fine : cloudy : fine. 25, 26. Cloudy : damp. 27. Damp. 28. Cloudy : fine : cloudy : damp. 29. Fog. 30. Rain : fog. 31. Damp : cloudy : damp.—This month has been remarkably fine and warm.

Mean temperature of July for twenty-five previous years	54 ·79
Mean temperature of this month	61°·36
Average quantity of rain in July for six years	2·71 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, *near London;* by Mr. Veall, at BOSTON; *and by the Rev. C. Clouston, at Sandwick Manse,* ORKNEY.

Days of Month. 1852. July.	Barometer — Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8½ p.m.	Thermometer — Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8½ p.m.	Wind — Chiswick 1 p.m.	Boston	Orkney, Sandwick	Rain — Chiswick	Boston	Orkney, Sandwick
1. ○	30·107	29·995	29·45	29·960	29·971	72	52	62·5	58	54½	sw.	w.	s.			·60
2.	30·131	30·099	29·53	29·946	29·965	73	50	63	52½	54	sw.	sw.	w.			·25
3.	30·158	30·117	29·60	29·987	29·988	80	51	66	55	58	s.	sw.	ese.			·01
4.	30·077	29·943	29·53	29·988	29·985	94	57	69	68½	65	s.	s.	se.			·10
5.	29·912	29·819	29·37	29·983	29·991	97	60	73	69½	62	s.	se.				
6.	29·866	29·808	29·30	29·994	30·004	90	61	74	59½	60	e.	se.				·19
7.	30·006	29·914	29·42	30·003	30·006	90	49	70	59½	60	e.	c.	se.			
8.	30·026	29·911	29·50	30·006	30·003	87	51	67·5	63	60	e.	c.	se.			
9. ☾	30·020	29·995	29·44	29·999	29·988	92	54	71	65½	57	e.	e.	se.			·30
10.	30·117	30·034	29·47	30·017	30·011	87	53	67	57	56	ne.	h.				
11.	30·127	30·049	29·47	30·009	30·017	81	57	71	59	57½	se.	ne.	e.			
12.	30·100	30·026	29·63	30·007	30·009	79	56	69	62	63	e.	ne.	se.			
13.	30·060	29·923	29·59	29·907	30·002	81	59	70	70	63	e.	ne.	se.			
14.	29·993	29·898	29·57	29·888	29·995	81	60	70	68	60	s.	c.	se.	·20		
15.	29·939	29·747	29·47	29·887	29·990	87	53	68·5	67	62	e.	sse.	se.	1·60		
16.	29·916	29·738	29·35	29·871	29·987	87	62	72·5	68	61½	sw.	calm		·01		
17. ●	29·811	29·820	29·35	29·881	29·972	73	52	70	65	61½	e.	s.	se.			
18.	29·890	29·945	29·20	29·971	29·931	79	50	71	66	61	sw.	w.	w.		·30	·15
19.	30·003	29·973	29·26	29·981	29·984	77	56	69	67½	59	sw.	ssw.	sw.		·34	
20.	30·034	29·942	29·37	29·987	29·985	74	54	66	66½	60	sw.	ssw.	w.			
21.	29·973	29·971	29·46	29·983	29·979	77	51	68·5	61	58	e.	w.				
22.	30·128	30·052	29·40	29·983	30·000	78	44	63	61½	60½	w.	w.	sse.	·01		·42
23.	30·138	29·842	29·47	29·805	29·995	79	52	73	64	61	e.	ene.	sse.			
24. ☽	29·951	29·656	29·62	30·005	29·986	78	57	73·5	59½	57	w.	se.	nw.	·36		
25.	29·733	29·640	29·47	29·980	29·989	74	58	70	58	57½	sw.	e.	n.	·10		
26.	29·708	29·862	29·26	30·005	30·014	77	59	70·5	62	59½	e.	c.	n.			
27.	29·993	29·993	29·14	30·012	30·020	79	52	66	62	60	ne.	w.	w.		·03	·14
28.	30·064	30·025	29·38	30·009	30·012	79	49	67·5	65	59	ne.	ne.	w.			·06
29.	30·054	30·034	29·52	30·006	30·010	74	47	66	62	60	w.	ene.	w.		·13	·08
30.	30·070	30·038	29·54	29·997	30·004	76	46	70	65	59	w.	nw.	w.			·18
31. ○	30·072	30·038	29·56	29·97	29·993	81	54	70	62	60	w.	calm				·10
Mean.	30·005	29·936	29·44	29·896	29·947	81·00	53·74	69·3	63·03	59·70				2·28	0·80	2·58

XXXVII. *Reports on the Progress of the Physical Sciences.*
By Dr. JOHN TYNDALL, *F.R.S.*

On the Stereoscopic Combination of Colours, and on the Influence of Bright-
ness on the relative Intensity of different Colours. By H. W. DOVE,
Berichte der Akademie, May 1851, and February 1852.

IN the July Number of this Journal for 1851 we gave a de-
scription of several forms of the stereoscope invented and
applied by Professor Dove. The inquiries in which these inven-
tions originated have been since followed up by the learned Pro-
fessor; and as they have reference to an application of this beau-
tiful, and now highly popular instrument, which has not as yet
been treated by its ingenious inventor, we propose placing the
results, in a condensed form, before the readers of the Philo-
sophical Magazine.

M. Dove's researches have reference chiefly to the stereoscopic
combination of colours. In 1841 he showed that the stereo-
scopic combination of the complementary colours of polarized
rays produced white light. He now makes use of drawings with
coloured outlines, the colours being either dioptric or catoptric:
the former he obtains by making drawings of white lines upon a
black ground, and viewing the stereoscopic combination through
a coloured glass; in the second case, the figures are drawn upon
white paper in the colours which are intended for combination.

The projection of a convex pyramid was drawn in red lines
upon a white ground, and on the same base the projection of a
concave pyramid in blue lines. On a second leaf the corre-
sponding drawings were made in the same colours for the other
eye. If, on viewing these drawings in the stereoscope, each pair
combined in the usual manner, we should have a convex red
pyramid and a concave blue pyramid, the axes of both forming

one and the same straight line.   But it is altogether impossible to obtain a relief in this case.   A hexagon embracing a six-pointed star is always obtained, the sides of which all consist of red and blue lines running alongside each other in contact. When this complicated figure was viewed through a blue glass, the convex relief, bounded by red lines, started forth ; when a red glass was used, the hollow pyramid with blue edges was observed. In the first case the blue lines vanished almost completely in the blue light; while the red, whose rays were intercepted by the glass, acted as black, and became subjectively coloured red.   In the second case the red lines nearly disappeared in the red light ; and the blue, subjectively coloured, combined themselves to a relief.

To understand what has been here said regarding subjective colouring, attention to the following facts is necessary.   If the diffused daylight be completely shut out from the eyes, and a drawing in black outline on white ground be viewed through a coloured glass, the relief is seen with black edges ; but when the glass is held at some distance from the eyes, so that the dif-fused daylight shall also reach them, the black hues assume a vivid subjective colouring, which becomes stronger the longer the drawing is regarded.   When the glass is coloured blue by cobalt, the lines appear red ; when the glass is a ruby-red, the outline appears bluish-green.

The result of the above experiment with the blue and red pyramids is remarkable.   Each eye has two drawings presented to it, and a double combination is thus possible.   When the identity of outline is preserved by the eye, and no regard is paid to the difference of tint, two plane figures composed of different colours must be observed.   This is the case when the intensity of both colours is nearly the same.   When, however, the intensity is very different, such, for example, as that brought about by the red and blue glasses in the case under consideration, the identity of the outline is overcome by the tendency to form a relief.

The projection for one eye was drawn in white lines upon a black ground, and for the other eye with black lines upon a white ground.   A most remarkable result was obtained by the stereo-scopic combination of both.   The relief started into existence with surfaces which shone like graphite, having their edges formed of dazzling white and deep black lines which run pa-rallel and in contact with each other throughout.   When the black leaf with the white lines is placed before the left eye, and the white leaf with the black lines before the right eye, the white lines in the relief lie to the right of the black ones.   When the leaves are changed, the relative position of the black and white hues is also changed ; hence the lines appear always pushed

aside cross-wise. Exactly as in the case of black and white, combinations of both of these with other colours are obtained, and combinations of the latter with each other. To obtain the combination of dioptric colours with white and with each other, drawings in white outline on a black ground are made use of. When white is to be combined with another colour, a glass of the required tint is placed before one eye, while the drawing is viewed by the other eye naked. When different colours are to be combined, suitable glasses are placed before both eyes. The most beautiful result is obtained when the colours produced by a deep blue and a red glass are combined; the relief stands forth illuminated with violet light and with splendid edges of red and blue, which run alongside each other in contact. In the case of colours which nearly approach each other, the edges are also formed by those double and differently coloured lines. One result is always observed,—the lines appear pushed aside cross-wise; that is, the colour observed by the left ·eye appears to the right, and that observed by the right eye appears to the left.

The following remarkable fact has been observed by M. Dove, and his observation has been corroborated by others. The projections of a convex and concave pyramid for the right eye were drawn upon the same base, and on a second leaf the projection of a convex only for the left eye. In the stereoscope, therefore, a convex pyramid was seen, and on the base of the same the projection of a concave one. When the ruby-red glass was brought before the left eye, while the former drawing was regarded by the naked right eye, both the pyramid and the projection were observed; but it depended entirely on an act of volition whether the pyramid was observed with red and white boundaries and the projection in white lines, or the pyramid with white boundaries and the projection in red and white outline. It hence appears that a projection as contour can combine itself with another as colour to form a relief.

The same phænomena which we have observed with objective colours exhibit themselves with subjective colours also. On viewing the drawings formed in black outline on white ground through the ruby-red glass with one eye, and through the glass coloured blue by cobalt with the other, permitting the diffused daylight at the same time to strike the eyes, the relief is observed with coloured double parallel lines as edges, as in the other instances: the crossed position of the lines is also observed here; so that when the red glass is held before the left eye, and the ‚blue glass before the right, the bluish-green lines appear to the right of the red;—it will be remembered that the subjective tint

developed by the red glass is bluish-green, and by the blue glass, red.

Why is it, then, that the red and blue lines cannot be made to combine, but always lie alongside each other crossed in the manner indicated ? M. Dove finds the explanation in the non-achromatic nature of the eye. That the eye is not achromatic has been known since the time of Fraunhofer ; but a very simple way of proving the fact was discovered independently by M. Dove and M. Plateau about twelve years ago. If the flame of a candle be viewed through a coloured glass which permits the ends of the solar spectrum to pass through it, but extinguishes the middle, at the distance of distinct vision a violet flame is observed. At a greater distance a red flame is observed within a larger blue one which embraces the former on all sides and becomes wider the further we recede from the flame. Within the distance of distinct vision, on the contrary, the violet flame is encompassed by a sharp red rim. From a medium distance a long-sighted eye sees the latter, and a short-sighted eye the former. Hence the experiment furnishes us with a kind of optometer ; to this purpose M. Dove has applied it in hundreds of cases, and never found a single individual whose eyes fulfilled the conditions of achromatism at all distances. Acquainted with this fact, and observing a certain analogy between it and his stereoscopic experiments, he naturally sought the cause of the phænomena presented by the latter in the non-achromatic nature of the eye.

A fine white line drawn upon a black ground was viewed through the glasses used in the stereoscopic experiments. It was ascertained that, to be plainly visible, it must be held at a greater distance from the eye when the red glass is used than when the blue glass is applied. Sir David Brewster has obtained an analogous result with pigments (Report of the British Association, 1848, p. 48). A number of square pieces of gradually decreasing size was cut from the same vividly-coloured card, and placed one upon the other so as to form a pyramid with ascending steps, all of the same height. Two such pyramids were built, the one beside the other ; the squares were blue and red ; one pyramid had a blue square for its base, the other a red one. It was always found that a blue square placed upon a red one appeared higher than a red square placed upon a blue one ; so that in the building of the pyramids, each appeared by turns to exceed the other in height. From this experiment it follows, that at the distance of distinct vision the lines of convergence of both eyes enclose a smaller angle in the case of red light than in the case of blue. Hence if an observer, who sees equally well with both eyes, have both colours presented to him in the ste-

reoscope in the manner already described, the lines cannot coincide, but will project themselves in directions which cross each other upon a surface which does not pass through the point of intersection of both directions.

M. Dove next goes on to consider the cause of the glistening, which, for example, is observed on the surface of varnished pictures, and which may be destroyed by quenching the polarized rays with a Nichol's prism. In every case in which a surface appears thus shining, there is a reflecting layer, more or less transparent, through which another body is viewed; the glistening owes its origin to the combination of the rays reflected from the surface and those which pass through the transparent layer from the body behind. This is increased when the number of alternations of the layers increases. Thus mica assumes a metallic lustre, and layers of glass plates the appearance of mother-of-pearl. In the projection of a truncated pyramid intended for a certain eye, the section was coloured with a saturated wash of blue; in the figure intended for the other eye, the section was coloured yellow. At the moment of combination, when the resultant green appeared, it seemed as if one layer of colour had become transparent and that the other was seen through it. When the coloured section was viewed through a violet glass held before both eyes, the surface appeared like polished metal.

These experiments are intimately connected with the phænomena of irradiation. They establish the fact, that the deportment of black and white towards the eye is exactly similar to that of two different colours. The lustre obtained by the combination of black and white is peculiarly strong; so decided, indeed, that some, and among others the writer of this report, compared it to the lustre of lead glance or of tin, although the component white and black were both perfectly dull and lustreless. According to the explanation already given, one of these surfaces must appear in advance of the other. The viewing of an object by the naked eyes by different degrees of illumination with white light is analogous to those experiments with coloured light, where the object, to be distinctly seen, must be brought nearer in the case of blue light than with red. A dark object will, under the same conditions, appear further off than a white one, as the red surface appears more distant than the blue. At the distance of distinct vision, the flame of a candle, when viewed through the violet glass, which permits the ends of the spectrum to pass and extinguishes the middle, appears violet; that is, the red flame is as large as the blue. At the distance of distinct vision, a white object also appears of the same size as a black one; at a greater distance, the blue flame embraces the red; that is, beyond the distance of

distinct vision the blue flame is larger than the red one ; and so also beyond this distance, the white object on black ground appears larger than the black object on white ground.   In this way the phænomena of irradiation are connected by a chain of experimental facts with chromatic phænomena, which directly point the way to the explanation of the former.   The complete explanation is embraced by the proposition, that for a given distance the capacity of accommodation of the eye is different for white and black.

In a recent paper M. Dove has added some proofs to those already given of the fact, that blue and red are plainly visible at different distances.   Beyond the point of distinct vision, a micrometer drawn in black lines upon a white ground appears as a gray spot, when drawn in white lines on a black ground it appears as a bright one.   If a series of parallel white lines be viewed through a blue glass, the observer gradually receding until the lines run into each other and are no longer distinct, from this distance the lines, if observed through a red glass, will appear quite distinct. The reader may in this way easily satisfy himself that the distance of distinct vision is considerably greater for red than for blue. In the same way it may be plainly shown that the distance for white is also greater than for blue.   It is difficult to obtain pigments of such equal intensity that their combination shall exhibit lustre, but the lustre can be readily obtained as follows :— A drawing in white lines upon a black ground is combined in the stereoscope with another in black lines upon a white ground, and viewed through a coloured glass held before both eyes. With the ruby-glass and bright light the relief appears like polished copper.   In this way we learn that the results, as regards lustre and irradiation, obtained with white and black, are also true for any colours whatever.

It is known that a green spot on a red field, which is moved quickly hither and thither, appears to oscillate.   Wheatstone has shown that a red heart on blue ground appears to oscillate still more quickly; hence the appearance is not to be referred to the action of complementary colours, but to a difference of refrangibility.   Sir David Brewster was the first to observe on geological maps that blue and red do not appear in the same plane, and the reason of this M. Dove considers to be rendered completely evident by his stereoscopic experiments.   His explanation of the fluttering heart is as follows :—When the sheet is moved in its own plane, the heart and the ground on which it rests describe tangents of the same absolute length, but with radii which the eye regards as different.   The angular velocities

of both thus appear to be different, and hence the object seems to oscillate upon the plane which bears it.

That yellow and red colours approach the nature of light more than blue is an idea which may be traced throughout antiquity. In the common language of the Germans, this is expressed by the terms 'screaming yellow,' 'burning red,' in contradistinction to 'deep blue.' This notion is corroborated by photometric experiments. But with these well-known phænomena, another stands apparently in complete contradiction. It has often occurred to M. Dove, on quitting a picture gallery on the approach of night, when he happened to cast a parting glance upon the paintings, the red colour had altogether disappeared while the blue appeared in all its strength. Artists are well aware of this fact; at least, on questioning such, M. Dove has always found his own observation corroborated.

The stereoscopic experiments already described furnish an accurate and beautiful method of observing this fact. On applying two glasses, one of which permits the homogeneous blue rays to pass, and the other the homogeneous red ones, the relief, as already stated, appears with beautiful edges of red and blue lines which run alongside each other. Although when the light is intense the red lines appear much the most vivid, the blue glass made use of being more than ten times the thickness of the red one, still as the twilight advances the red becomes weaker and weaker; it finally disappears altogether, and instead of the relief formed by the combination of the red and blue outline, the blue alone is observed, as projection, upon its proper leaf. If two red glasses be now placed before the openings of the stereoscope, nothing whatever is seen; while with two blue glasses the relief appears in blue lines, and remains distinctly visible for a quarter of an hour longer. Thus the fact of the earlier disappearance of the red rays is placed beyond a doubt :—how is this to be accounted for?

It is known that weak impressions on the organs of sense singly may arouse no consciousness, but do so where they are quickly and uniformly repeated. On this account the string of the contra-basso must have a wider amplitude than that of the violin, inasmuch as the diminished number of vibrations demands a greater energy to render them heard. Thus also if we wish to make ourselves heard without great effort, we speak in a higher tone; and hence it is that when the deep voice of the seaman, strengthened by the speaking trumpet, is lost in the storm, the shrill pipe of the boatswain still pierces through the howl of winds and roar of waves. Savart has shown, by means of the toothed-wheel, that the limit of sensibility of the ear for grave tones is extended by strengthening the strokes. The complete similarity

of the vibrations causes the most perfect summation of impressions, because the interferences which take place when the times of oscillation are different then fall away. This uniformity renders the tone pure, and, in the case of colours, renders them homogeneous. Blue stands in the same relation to red that a higher tone occupies with regard to a deeper one. With blue the vibrations of the retina are more frequent than with red, as the vibrations of the tympanum are more frequent with a high tone than with a deep one. Now it is proved that with deep tones the limit of sensibility becomes contracted when the tones become weaker; and this is completely analogous to the case, that by decreasing brightness, the limit of sensibility for the red rays should become narrower. Hence with weak illumination, red, as a colour, disappears; while blue, on account of the greater frequency of its vibrations, remains longer visible.

"In this way," observes the Professor, "I explain to myself the wonderful phænomenon, regarding which, however, strange to say, nobody has expressed wonder, that by the weak light of the stars the blue of the firmament is rendered distinctly visible."

Herewith is connected the fact, that a prismatic spectrum obtained from light which has passed through a narrow aperture has its colours towards the red end comparatively stronger when the light is intense. This is peculiarly plain if the spectrum be viewed through a dichromatic medium which permits the ends of the spectrum to pass and extinguishes its middle, thus enabling both ends to be immediately compared with each other. The dark space beyond the red end of the spectrum, where the calorific effect is a maximum, would probably become distinctly visible if the intensity of the sunlight were considerably increased by concentration. This would be the experiment of Savart applied to colours. Probably to the subject we are considering belong the experiments of Sir David Brewster on the lines of Fraunhofer in this portion of the spectrum; although the facts observed appear to be referred to the destruction of spherical aberration, and not to the illuminating power of the telescope applied. In a similar manner the limits of action on an iodized silver plate at the violet end of the spectrum become expanded with increasing brightness.

If a person pass suddenly from a brightly illuminated room into a very dark one, and then approach the place through which the light enters until blue becomes distinct, it will be found that red is at first much more vivid. The eye must remain for some time in the darkened room before the retina becomes so sensitive as in deep twilight. When this is attained, the person may recede to a distance from the place where the light enters where the blue is still distinctly visible, and find that the red has

vanished completely.   Another remarkable fact observed by
M. Dove was, that among the numbers to whom he showed, in
bright daylight, the stereoscopic relief with blue and red edges,
one declared that he saw only the drawing with blue lines, as
through the red glass he could see nothing whatever.   The eyes
of this individual in bright daylight were in the same condition
as a pair of normal eyes by twilight.

XXXVIII. *On Atmospheric Electricity, according to the Obser-
vations at Munich and Brussels.   Letter of* M. Quetelet *to*
M. Lamont, *Director of the Observatory at Munich**.

I HAVE long upbraided myself for not having replied to your
obliging letter, wherein you requested me to make compa-
rative observations on the electricity of the air.   My purpose
was to request you, in the first place, to give me some instruc-
tions relative to the instruments which you have made use of,
and to the results at which you have arrived, so as to assure
myself that our observations might be compared with each other.
I have been partially able to satisfy my desire in this respect by
reading your description of the instruments used at Munich
which you have been kind enough to send me, as well as the
article inserted in the 4th Number of Poggendorff's *Annalen*
for 1852.

In running over the table of your observations from 1850 to
1851, I have been struck with the small resemblance which sub-
sists between your numbers and those obtained at Brussels : to
enable you to judge of this, I will set side by side the monthly
results which you give for the hour of noon, and those which I
have obtained myself for the same hour.   Your results are con-
tained in the second column, a, of the following table ; mine are
contained in the third column, b.   You have seen from my first
investigation, published in the month of June 1849, that the
numbers immediately observed by the electrometer of Peltier do
not express the absolute values of the electric tension which are
given in the following column, b', according to each day's reduced
observations ; hence the last numbers are those which ought to
be compared with yours.   In order to facilitate the comparison,
I have reduced all the values to the same unit, to the monthly
mean deduced from the results of the twelve last months which
occur in the table in the columns a, β, and β'.

* From vol. ix. of the *Bulletins de l'Académie Royale de Belgique*.
Communicated by the Author.

Months.	Observed numbers.			Reduced numbers.		
	Munich.	Brussels.		Munich.	Brussels.	
	a.	b.	b'.	α.	β.	β'.
May (1850) ...	3·08	19	145	0·72	0·62	0·91
June	2·80	14	25	0·65	0·45	0·16
July	3·28	12	22	0·76	0·39	0·14
August	3·72	22	84	0·87	0·71	0·52
September	3·23	28	96	0·75	0·91	0·60
October	4·88	36	153	1·14	1·17	0·96
November	5·51	35	162	1·28	1·14	1·01
December	7·20	45	272	1·68	1·46	1·70
January (1851).	6·34	50	440	1·48	1·63	2·78
February.........	5·98	51	470	1·39	1·66	2·93
March............	5·18	28	106	1·21	0·91	0·66
April	3·04	27	95	0·71	0·88	0·59
May	2·56	21	53	0·60	0·68	0·33
June	3·11	19	45	0·72	0·62	0·28
July	3·15	20	50	0·73	0·65	0·31
August	3·03	21	53	0·71	0·68	0·33
September	2·85	24	65	0·66	0·78	0·41
October	3·59	29	104	0·83	0·94	0·65

If Munich and Brussels were in the same electric condition, the numbers α and β' would be equal, or at least would present the same fluctuations. Thus, similar to all physicists who have examined atmospheric electricity, we find that the electric tension is stronger in winter than in summer; but the ratio which you obtain is hardly that of 2 to 1, while for Brussels it is about 9 to 1. Is this enormous difference due to local causes? I hardly believe it. As you have not published, up to the present time, the summary of your observations, and have not entered into details with regard to the manner according to which your means were calculated, I am not aware if all the observations without distinction have been brought into the calculation or not.

In this state of doubt I was desirous to compare our results with others obtained in different localities; unhappily, however, I know but one single series of observations on this interesting but neglected portion of meteorology; these are the observations made at Kew by Mr. Ronalds from 1845 to 1847*. I give them in the following table with the general results of Brussels, for the seven years from 1845 to 1851. The observations at Munich, Brussels and Kew, have reference to the hour of noon; they have been rendered comparable in three special columns by taking for unity the monthly mean.

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* Report of the 19th Meeting of the British Association held at Birmingham in September 1849; see the memoir of Mr. Birt, p. 113.

Months.	Observed numbers.			Proportional numbers.			Brussels.	
	Brussels	Kew.	Munich.	Brussels.	Kew.	Munich.	Proportional numbers.	Observed numbers.
January ...	518	182·4	6·34	2·82	2·40	1·48	1·61	50
February...	333	179·3	5·98	1·81	2·35	1·39	1·45	45
March......	169	58·2	5·19	0·92	0·76	1·21	1·13	35
April	105	40·7	3·04	0·57	0·54	0·71	0·77	24
May	81	41·3	2·56	0·44	0·55	0·60	0·65	20
June	40	26·8	3·11	0·22	0·35	0·72	0·55	17
July	42	31·8	3·15	0·23	0·42	0·73	0·55	17
August ...	62	28·5	3·03	0·34	0·38	0·71	0·68	21
September.	74	31·0	2·83	0·40	0·41	0·66	0·81	25
October ...	140	65·1	3·59	0·76	0·85	0·83	1·03	32
November .	230	80·5	5·51*	1·25	1·34	1·28	1·29	40
December .	412	126·3	7·20*	2·24	1·65	1·68	1·48	46
Year	184	74·3†	4·29	12·00	12·00	12·00	12·00	31

The result of these observations is, that the electric tensions in winter and summer are to each other, for Brussels as 9 to 1, for Kew as 6 to 1, and only as 2 to 1 for Munich. Differences of such magnitude, if they really exist, possess the highest scientific interest; if they are due to the imperfection of the instruments, they merit scarcely less attention.

It is essential, in the first place, to examine if the cause of these differences resides in the manner in which the observations have been collected, or in their mode of calculation.

Although applying the instrument of Peltier, with some modifications, you have pursued a different method from that followed by the above-named physicist to render your results comparable. M. Peltier estimated the value of the degrees of his instrument by transferring the electric charges *directly* to Coulomb's balance; and he indicated by a table the electric tension corresponding to every angle of deviation ϕ of the moveable needle of his electrometer.

I have employed a similar table based upon a principle somewhat different, that of dividing the electricity between two balls of equal surface. I have found that the table calculated in this manner for the degrees of my electrometer, agrees perfectly with that calculated by Peltier from his experiments for the same instrument. The two methods of experiment thus exhibit the same results.

You have preferred following another way: you have had recourse to calculation; and, admitting the hypothesis that *the electricity is uniformly distributed in the conductor and in the moveable needle,* you find that the electric tension η is very nearly proportional to the angle ϕ; so that we may take $\eta = \phi + \mathrm{F}(\phi)$,

* These numbers belong to 1850, the preceding to 1851.
† The notice gives the number 75·4, which is not the mean of the year.

where $F(\phi)$ represents a small correction dependent on the angle ϕ. You consider that this correction, and that due to the torsion of the fibre, may be neglected through the extent of an arc of about 65°, which is represented by nine divisions of your scale.

This result of your calculation does not agree with the results deduced from observation by M. Peltier and me, even for feeble electric tensions. In admitting it, the values β and β^l of the first table relative to Brussels would be sensibly equal, which is far from being the case. This is an essential point, to which I permit myself to direct your attention.

Taking, with you, the values directly observed at Brussels as representing the electric tensions of the air *without applying any correction*, I find that my numbers come very near to yours, and that the ratio of summer to winter is less than that of 3 to 1; but is this substitution legitimate?

Permit me to submit to you one other observation: you say at the fifth page of the description of the new instruments and apparatus at the Observatory of Munich, that the electrometer which you have made use of is constructed after the principle of the instrument of Peltier used at the Observatory of Brussels; but that the method pursued to determine the electric tensions of the air from the readings of the instrument is essentially different. I find, in fact, in your description all the principal parts of the electrometer which has served for my observations, and which was constructed for our observatory by M. Peltier; I remark, however, one important difference in the proportions: the ball which surmounts my instrument is considerably larger than yours, at least if I may infer from the drawing, for you have given no dimensions.

I could have desired to know the motives which have induced an observer so skilful as yourself to reduce the ball to a dimension so small relatively to the stem which it surmounts; it seems to me that this reduction must have for its effect a reduction of the sensibility of your apparatus. It is in this sense that M. Peltier has remarked, that the induced electricity coerced at the extremity of the stem leaves to that of the contrary name the rest of the length whereon to distribute itself; but the longer the stem is relatively, the less will be the portion of it which returns to the indicating needle, and the less will be the divergence.

You will excuse me, my dear confrère, for thus submitting to you my doubts. It appears to me of the greatest importance to recognise the true cause of the errors, if such exist; and I do it with all the confidence with which your talents, and the love of truth which animates both of us, inspire me.

Brussels, August 5, 1852.

XXXIX. *On the state of Static and of Dynamic Electricity during several heavy Showers observed at Brussels on the 14th of June 1852. By* A. QUETELET, *Membre de l'Académie Royale de Belgique**.

I KNOW but few observations made simultaneously during rains and storms on the static and the dynamic electricity of the air. These two meteorological elements are, notwithstanding, of the greatest importance, and rarely march together; that is to say, during powerful electric tensions it often occurs that no current is observed; and, on the contrary, very decided currents sometimes exist while the electrometer exhibits nothing extraordinary.

The showers of which I am about to render an account have presented some peculiarities which appear to me to be worthy of attention. The dynamic electricity was observed by means of a very sensible Gourjon's galvanometer; one of the wires was connected with the earth, and the other with a conductor placed on the roof of the observatory. The static electricity was observed by means of the atmospheric electrometer of Peltier; the observations were made on the summit of one of the turrets of the observatory, and on a small platform placed at an altitude higher than the surrounding objects.

On the 14th of June 1852 it had rained at different intervals during the morning; 2·55 millimetres of water were collected. Towards noon thick clouds floated in the inferior regions of the atmosphere, between which portions of the heaven and of *cumuli* were visible, whose splendid white contrasted with their gray and slightly copper-coloured hues. The Centigrade thermometer marked 13°·6 and the barometer 739·73 millimetres. The pressure of the atmosphere was passing a state of minimum at the time. The direction of the clouds, in accordance with that of the weathercock, indicated a moderate wind in the direction of W.S.W.

The electrometer of Peltier, interrogated at various times and at intervals of 2 to 3 minutes, indicated successively $-19°$, $-30°$, $-35°$, $-40°$, $-30°$. A shower was observed to the W.S.W., and during the last observation a small cloud which crossed the zenith let fall some drops of rain. It was then about 10 minutes past 12 o'clock at noon, and the nimbus caused by the rain to the W.S.W. approached insensibly.

I descended to invite M. Bouvy, one of my assistants, to follow the indications of the galvanometer of Gourjon, while I, by means of Peltier's electrometer, might continue my observations on the summit of one of the turrets of the observatory; my object being

to form a judgement of the respective states of the static and dynamic electricity of the air during the fall of rain which would soon take place. I then quickly reascended.

Towards $12^h 15^m$ I recommenced my electric observations, which I continued at intervals of 2 to 3 minutes; I obtained successively $-46°$, $-57°$, $-61°$, $-64°$, $-65°$; during this last observation, the nimbus, which approached more and more, touched the zenith by its nearest edge; the wind became very sensibly increased, and the first drops of rain commenced to fall; the electrometer indicated $-69°$; and at the moment when the shower descended $-75°$, it was $12^h 33^m$; two minutes afterwards the rain fell with less violence, and the electrometer marked $-74°$, then $-73°$. At $12^h 37^m$ the darkest portion of the nimbus had passed the zenith, and the rest of the cloud yielded no more water; but a fresh shower was forming itself to the south and south-east, the electrometer marked $0°$; consulted immediately afterwards it indicated $+75°$. I would have observed the time, but observed with astonishment that my watch had stopped.

While the rain which set in to the south extended itself to Brussels, but yielding very little water, it continued to develop itself with intensity towards the horizon, while in the mean time new showers were forming to the east, the north-east, and north. I estimate that the hour was about $12^h 48^m$; the rain-cloud which was in the zenith enlarged itself, and yielded water for a few minutes; the electrometer continued to be observed and ceased not to indicate $+75°$, the highest degree to which it attained[*].

At a little past 1 o'clock the last edge of the cloud touched the zenith; the sun shone at intervals; the rain was still very heavy between the S. and E.N.E., the electrometer had not ceased to indicate $+75°$; a little after it descended to $+72°$, the zenith commenced to clear itself; the clouds floated away in different directions; the wind in the inferior regions was still between the S.W. and the W.S.W.; and the rain-clouds formed to the S.E. approached; their edges were strongly indented.

Towards $1^h 10^m$ I descended and received the observations made by M. Bouvy, who being obliged to leave, had ceded his place to another observer. Here are the indications which he obtained from the galvanometer while I collected those of the electrometer.

Up to two minutes after the commencement of the rain, the galvanometer had not ceased to preserve its ordinary position of

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[*] In the course of repairs recently made on the instrument, the scale over which the needle moved was found to be a little contracted. I always remarked, that by the rapidity of the oscillations of the needle I could judge of the moment when the electric tension was a *maximum*.

equilibrium at 5° A *; the needle commenced to move at 12ʰ 35ᵐ, and oscillated between 19° B and 1° A; at 12ʰ 35ᵐ its oscillation extended through an arch comprised between 1° B and 34° B, then between 30° B and 10°·5 A. At 12ʰ 36ᵐ·5 the rain ceased, and the needle oscillated round its ordinary position of equilibrium from 0° to 10° A; afterwards from 2°·5 to 9° A; and finally came to rest at 5°·5 A.

A descending current was thus exhibited, but only during the descent of the rain, and the needle was brought to a state of repose at the moment when the electricity changed its sign in such a remarkable manner. The oscillations recommenced at 12ʰ 48ᵐ, contemporaneous with the second rain, which was very light and of very short duration; the first impulse carried the needle from 8° to 12° A, it then oscillated round its position of equilibrium from 1° to 8° A, then from 3° to 7° A; the direction of the current had changed, it was now ascending. A new change took place afterwards; the needle oscillated from 5° B to 4° A, then from 2° B to 4° A up to 1 o'clock, when it was again arrested at 5° A.

I was particularly astonished to learn that the watch of M. Bouvy had stopped almost at the same instant as my own, that is at 12ʰ 37ᵐ, at the moment when the sudden change of sign of the atmospheric electricity took place. Was it accidental, or an effect of the electricity? This it will be difficult to decide; I confine myself to the statement of facts.

Setting out from 1ʰ 15ᵐ, the galvanometer was continually observed, but it did not forsake its position of equilibrium; I returned to my electrical observatory and found the electrometer always indicating +75°. The clouds continued to move in different directions; they were observed to advance towards each other, to stop, to attract each other, and then mingle together. The rain-clouds which came from the S.E. united themselves insensibly with others from the N.W., the electrometer marked +72°. The zenith became overcast, some drops fell, +73°; then at 1ʰ 24ᵐ the rain turned towards the E. +72°. The clouds towards the zenith and the S.W. were so thin as to permit of the solar disc being seen through them, +64°.

At 1ʰ 28ᵐ a little rain; the clouds were directed from the S.W. to the N.E. in the sense indicated by the weathercock. The electrometer indicated +61°; we continued to have glimpses of the sun. At 1ʰ 34ᵐ the sun reappeared, the zenith became clear, and the electrometer marked zero; the rain had turned to the east.

At 1ʰ 56ᵐ the zenith became again charged; the electrometer

* When the head of the needle points towards B, the current is descending; when towards A, the current ascends.

indicated successively $-2°$, $-18°$, $-28°$, $-15°$.   At 1^h 45^m the rain still fell in different directions, but not at Brussels; the sun shone at intervals, and the electrometer marked $-6°$.

I ought to remark, that during the showers not a single peal of thunder was heard, and not the smallest flash of lightning was visible.

The example which I have just cited shows how, during the same shower, according to the instant at which an observation is made, we may obtain either positive or negative electricity; this electricity is very energetic during the showers.   If the observation is made at the moment when the sign changes, it may appear to be nearly null; these inversions, it may be remarked, are always of short duration.

XL. *On the Mechanical Action of Radiant Heat or Light*: *On the Power of Animated Creatures over Matter*: *On the Sources available to Man for the production of Mechanical Effect.* *By* Professor WILLIAM THOMSON.*

On the Mechanical Action of Radiant Heat or Light.

IT is assumed in this communication that the undulatory theory of radiant heat and light, according to which light is merely radiant heat, of which the vibrations are performed in periods between certain limits of duration, is true. "The chemical rays," beyond the violet end of the spectrum, consist of undulations of which the full vibrations are executed in periods shorter than those of the extreme visible violet light, or than about the eight hundred million millionth of a second.   The periods of the vibrations of visible light lie between this limit and another, about double as great, corresponding to the extreme visible red light.   The vibrations of the obscure radiant heat beyond the red end are executed in longer periods than this; the longest which has yet been experimentally tested being about the eighty million millionth of a second.

The elevation of temperature produced in a body by the incidence of radiant heat upon it is a mechanical effect of the dynamical kind, since the communication of heat to a body is merely the excitation or the augmentation of certain motions among its particles.   According to Pouillet's estimate of heat radiated from the sun in any time, and Joule's mechanical equivalent of a thermal unit, it appears that the mechanical value of the solar heat incident perpendicularly on a square foot above

the earth's atmosphere is about eighty-four foot-pounds per second.

Mechanical effect of the statical kind might be produced from the solar radiant heat, by using it as the source of heat in a thermo-dynamic engine. It is estimated that about 556 foot-pounds per second of ordinary mechanical effect, or about the work of " one horse power," might possibly be produced by such an engine exposing 1800 square feet to receive solar heat, during a warm summer day in this country ; but the dimensions of the moveable parts of the engine would necessarily be so great as to occasion practical difficulties in the way of using it with œconomical advantage that might be insurmountable.

The *chemical* effects of light belong to the class of mechanical effects of the statical kind ; and reasoning analogous to that introduced and experimentally verified in the case of electrolysis by Joule, leads to the conclusion that when such effects are produced there will be a loss of heating effect in the radiant heat or light which is absorbed by the body acted on, to an extent thermally equivalent to the mechanical value of the work done against forces of chemical affinity.

The deoxidation of carbon and hydrogen from carbonic acid and water, effected by the action of solar light on the green parts of plants, is (as the author recently found was pointed out by Helmholz* in 1847) a mechanical effect of radiant heat. In virtue of this action combustible substances are produced by plants ; and its mechanical value is to be estimated by determining the heat evolved by burning them, and multiplying by the mechanical equivalent of the thermal unit. Taking, from Liebig's Agricultural Chemistry, the estimate 2600 pounds of dry fir-wood for the annual produce of one Hessian acre, or 26,910 square feet of forest land (which in mechanical value appears not to differ much from estimates given in the same treatise for produce of various kinds obtained from *cultivated* land), and assuming, as a very rough estimate, 4000 thermal units Centigrade as the heat of combustion of unity of mass of dry fir-wood, the author finds 550,000 foot-pounds (or the work of a horse-power, for 1000 seconds) as the mechanical value of the mean annual produce of a square foot of the land. Taking 50° 34′ (that of Giessen) as the latitude of the locality, the author estimates the mechanical value of the solar heat which, were none of it absorbed by the atmosphere, would fall annually on each square foot of the land, at 530,000,000 foot-pounds ; and infers that probably a good deal more, $\frac{1}{1000}$ of the solar heat, which actually falls on growing plants, is converted into mechanical effect.

* *Ueber die Erhaltung der Kraft*, von Dr. H. Helmholz. Berlin, 1847. [A translation of this essay will appear in the First Part of the New Series of the Scientific Memoirs.—Ed.]

When the vibrations of light thus act during the growth of plants, to separate, against forces of chemical affinity, combustible materials from oxygen, they must lose *vis viva* to an extent equivalent to the statical mechanical effect thus produced; and therefore quantities of solar heat are actually put out of existence by the growth of plants, but an equivalent of statical mechanical effect is stored up in the organic products, and may be reproduced as heat, by burning them. All the heat of fires, obtained by burning wood grown from year to year, is in fact solar heat reproduced.

The actual convertibility of radiant heat into statical mechanical effect, by inanimate material agency, is considered in this paper as subject to Carnot's principle; and a possible connexion of this principle with the circumstances regarding the quality of the radiant heat (or the colour of the light), required to produce the growth of plants, is suggested.

On the Power of Animated Creatures over Matter.

The question, "Can animated creatures set matter in motion in virtue of an inherent power of producing mechanical effect?" must be answered in the negative, according to the well-established theory of animal heat and motion, which ascribes them to the chemical action (principally *oxidation*, or a combustion at low temperatures) experienced by the food. A principal object of the present communication is to point out the relation of this theory to the dynamical theory of heat. It is remarked, in the first place, that both animal heat and weights raised or resistance overcome, are *mechanical* effects of the chemical forces which act during the combination of food with oxygen. The former is a dynamical mechanical effect, being thermal motions excited; the latter is a mechanical effect of the statical kind. The whole mechanical value of these effects, which are produced by means of the animal mechanism in any time, must be equal to the mechanical value of the work done by the chemical forces. Hence, when an animal is going up-hill or working against resisting force, there is less heat generated than the amount due to the oxidation of the food, by the thermal equivalent of the mechanical effect produced. From an estimate made by Mr. Joule, it appears that from $\frac{1}{4}$ to $\frac{1}{6}$ of the mechanical equivalent of the complete oxidation of all the food consumed by a horse may be produced, from day to day, as weights raised. The oxidation of the whole food consumed being, in reality, far from complete, it follows that a less proportion than $\frac{5}{6}$, perhaps even less than $\frac{3}{4}$, of the heat due to the whole chemical action that actually goes on in the body of the animal, is given out as heat. An estimate, according to the same principle, upon very imperfect data, however, is made by the author, regarding the relation between the thermal and the non-thermal mechanical effects produced by a man at

work; by which it appears that probably as much as $\frac{1}{5}$ of the whole work of the chemical forces arising from the oxidation of his food during the twenty-four hours, may be directed to raising his own weight, by a man walking up-hill for eight hours a day; and perhaps even as much as $\frac{1}{4}$ of the work of the chemical forces may be directed to the overcoming of external resistances by a man exerting himself for six hours a day in such operations as pumping. In the former case there would not be more than $\frac{5}{6}$, and in the latter not more than $\frac{3}{4}$ of the thermal equivalent of the chemical action emitted as animal heat, on the whole, during the twenty-four hours, and the quantities of heat emitted during the times of working would bear much smaller proportions respectively than these, to the thermal equivalents of the chemical forces actually operating during those times.

A curious inference is pointed out, that an animal would be sensibly less warm in going up-hill than in going down-hill, were the breathing not greater in the former case than in the latter.

The application of Carnot's principle, and of Joule's discoveries regarding the heat of electrolysis and the calorific effects of magneto-electricity, is pointed out; according to which it appears nearly certain that, when an animal works against resisting force, there is not a *conversion of heat into external mechanical effect*, but the full thermal equivalent of the chemical forces is *never produced*; in other words, that the animal body does not act as a *thermo-dynamic engine*; and very probable that the chemical forces produce the external mechanical effects through electrical means.

Certainty regarding the means in the animal body by which external mechanical effects are produced from chemical forces acting internally, cannot be arrived at without more experiment and observation than has yet been applied; but the relation of mechanical equivalence, between the work done by the chemical forces, and the final mechanical effects produced, whether solely heat, or partly heat and partly resistance overcome, may be asserted with confidence. Whatever be the nature of these means, consciousness teaches every individual that they are, to some extent, subject to the direction of his will. It appears, therefore, that animated creatures have the power of immediately applying, to certain moving particles of matter within their bodies, forces by which the motions of these particles are directed to produce desired mechanical effects.

On the Sources available to Man for the production of Mechanical Effect.

Men can obtain mechanical effect for their own purposes either by working mechanically themselves, and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air,

and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes :—

   I. The food of animals.

   II. Natural heat.

   III. Solid matter found in elevated positions.

   IV. The natural motions of water and air.

   V. Natural combustibles (as wood, coal, coal-gas, oils, marsh gas, diamond, native sulphur, native metals, meteoric iron).

   VI. Artificial combustibles (as smelted or electrolytically deposited metals, hydrogen, phosphorus).

In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions :—

1. *Heat radiated from the sun* (sunlight being included in this term) *is the principal source of mechanical effect available to man*[*]. From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, and galvanic engines, and part at least of the mechanical effect obtained by means of windmills and the sails of ships not driven by the trade-winds.

2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly, no doubt, from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides. The mechanical effect so largely used in the sailing of ships by the trade-winds is derived partly, perhaps principally, from the earth's motion of rotation, and partly from solar heat.

3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body,—or meteoric,—that is, belonging to bodies deposited on the earth from external space. Terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of all inorganic native combustibles, are actually used, but the mechanical effect obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes.

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[*] A general conclusion equivalent to this was published by Sir John Herschel in 1833. See his Astronomy, edit. 1849, § (399).

XLI. *On a new Fossil Resin.* By J. W. MALLET, *Ph.D.*[*]

THIS mineral is from the coal measures in the neighbourhood of Wigan; of its exact locality, however, I am ignorant, as well as of the nature of the matrix in which it was found, as the specimen which came into my hands was quite detached.

It occurs massive in small drops or tears of round and ovoid forms, varying from the size of a pea to that of a hazel nut. Sometimes two or three of these drops are stuck together. Brittle; breaking with a very distinct conchoidal fracture. Colour—by reflected light black—by transmitted dark reddish-brown. Streak, cinnamon-brown. Translucent only in very thin splinters. Lustre, between vitreous and resinous, rather brilliant. Spec. grav. = 1·136. Hardness = 3. Possesses a slight resinous odour when pulverized.

Heated on platina foil it swells up, takes fire like pitch, and burns with a disagreeable empyreumatic smell and a smoky flame, leaving a coal rather difficult to burn, and finally a little gray ash. When heated in a glass tube closed at one end it yields a little water, swells up, melts, giving off a large quantity of a yellowish-brown oily product of a nauseous empyreumatic smell, and leaves a residue of carbon. It is insoluble in water, alcohol, æther, caustic and carbonated alkalies, or dilute acids. Even strong nitric acid acts on it but slowly. For analysis it was finely pulverized, boiled in successive portions of water, alcohol and æther (by which treatment traces of a substance resembling an oil were removed), and then dried at about 250° F.

9·52 grs. of the resin thus purified left on incineration ·35 gr. of ash (=3·68 per cent.), consisting principally of carbonate of lime, with traces of silica, alumina, and peroxide of iron. Most of the drops were coated externally with a little peroxide of iron, but this had been removed before proceeding to the chemical examination. A portion of the mineral was tested for nitrogen, but none was indicated. For the determination of the carbon and hydrogen, chromate of lead was employed; by combustion with which 6·32 grs. gave 17·78 grs. of carbonic acid and 5·04 of water; and in a second experiment, 6·96 grs. gave 19·68 carbonic acid and 5·68 water.

From these numbers the per-centage composition of the resin turns out to be—

Carbon	.	.	.	.	76·74	77·15
Hydrogen	.	.	.	.	8·86	9·05
Oxygen	.	.	.	.	10·72	10·12
Ash	.	.	.	.	3·68	3·68
					100·00	100·00

These numbers agree very well with the formula $C^{10} H^7 O$, or $C^{40} H^{28} O^4$, taking the number of atoms of carbon at 40, the number to which the composition of so many of the other resins, both fossil and recent, seems to be referable. By calculation from this formula the resin should contain in 100 parts,—

Carbon	77·05
Hydrogen . . .	8·99
Oxygen	10·28
Ash	3·68

This mineral approaches amber (as a single compound $C^{40}H^{32}O^4$) in composition more nearly than any of the other fossil resins, differing from it by four atoms of hydrogen if the higher number of equivalents of carbon be adopted for both, or only one atom if we take the smallest numbers expressing their constitution.

From the hardness of the mineral exceeding that usual in others of the same class in a considerable degree, it might be called Scleretinite ($\sigma\kappa\lambda\eta\rho\grave{o}\varsigma$-$\dot{\rho}\eta\tau\acute{\iota}\nu\eta$) in the absence of any more appropriate term.

XLII. *On Copper Smelting.* By James Napier, *F.C.S.**
[Continued from p. 201.]

On the Fuel suitable for Copper Smelting.

IT has been remarked, that wherever iron ore abounds, coal and lime as fuel and flux are contiguous. The opposite relations with regard to the fuel is observable in the case of ores of copper; where these ores are abundant, coals are not found; it is therefore of great consequence that a coal district be sought so contiguous that the cost of transit of either coal or ore should not interfere with the full development of the mineral wealth of either locality. Such is eminently the condition of the Swansea coal district in relation to the Cornwall and Devonshire copper mines.

What first induced the old copper smelters to erect their works in Swansea rather than Cornwall, we do not at present know. The average per-centage of copper in the ore being 8, and 20 tons of coal being required in the early period of smelting to produce one ton of copper, it was cheaper to take $12\frac{1}{2}$ tons of ore to Swansea than 20 tons of coal to Cornwall. And Cornwall requiring coal for domestic purposes and her tin smelting, which by law must be smelted in the county, ensured a back freight. These were no doubt some of the causes which deter-

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mined and maintained Swansea as a proper locality for copper smelting; besides, the quality of the coal found in the Swansea district is of the best for smelting purposes.

Smelting of copper ores commenced in Swansea upwards of 120 years ago, and has since steadily increased. About 30 years ago there were 8 smelting-works in and around Swansea; now there are 15, and 1 in course of erection, having in all about 500 furnaces in operation. There is also a copper smelting-work in Lancashire near Liverpool, and one in Anglesea.

The coals used for smelting copper are what are termed *strong and free burning* coal; they give a very great heat and considerable flame. Those of South Wales are of superior quality for the purposes of smelting, and the quantity is almost inexhaustible. Upon this subject, Mr. Josiah Richardson, in a paper read to the Society of Civil Engineers, has the following remarks :—" When the coal-field of South Wales should be brought into full work, the duration of the supply was beyond calculation. The area of this coal-field alone he estimated, from actual survey, to be 1055 square miles, embracing all qualities, from extremely bituminous coal to pure anthracite. The various veins and their several thickness were fully described, with examples of their quality, and analysis of them chemically, with their practical evaporating powers—showing that there existed 64 seams or veins of coal, having an aggregate thickness of 190 feet. These veins were described to be so situated as to be easily worked by adits or levels, and by pits of slight depth; and thus the cost at the mouth of the levels varied from 2s. 2d. to 3s. 6d. per ton—giving a mean about 2s. 10d. per ton. The actual annual consumption was shown to be—

		Tons.
In the iron-works of South Wales	. .	1,500,000
The copper-works	. .	300,000
The tin-plate and other works . .	. .	200,000
In agricultural and domestic uses	. .	1,000,000
Exports	. .	1,500,000
Total		4,500,000."

The quality of the coal for smelting purposes varies considerably. Anthracite coal has not yet been made available for smelting by reverberatory furnaces; and many qualities of that termed free burning or bituminous coal cannot be used alone, being what is termed too weak, while others are considered too strong; and under these two classes or designations, all the coals used in the smelting works are ranged. The two qualities of coal, strong and weak, differ considerably in price, and are often the produce of one pit, the smelting master being in many instances

the proprietor: but it is always of considerable importance that both qualities of coal should be used; therefore a judicious mixture of the two is a matter where œconomy in the production of copper can be practised, and, as we shall have occasion to notice, is often necessary for the protection of the materials of the furnaces.

The first question which naturally arises is, What constitutes the strength of the coal as applied to the purposes of copper smelting? Are strong coals those which have been found to give the strongest heat in a blast or cupola furnace, or the greatest evaporating power when under a boiler? Anthracite coal, which is ranked highest in a blast or for evaporating purposes, is ranked amongst the weak coal in the copper-works. These coals give no flame; the heat is not carried over the chamber or bed of the furnace; the part nearest the fire may be at a smelting heat, while that furthest off is only of a dull red. All good coals for smelting are capable of giving a certain amount of flame, although wherever there is much flame there is not much heat; neither is the heat great while the flame is passing over the furnace, it being absorbed by the expansion of the burning gases constituting flame; but while these gases are burning off the coals are expanding, becoming porous and spongy, admitting air freely through the mass; so that when the blaze of flame is past, the fire gives the strongest and most equable heat over the materials in the hearth.

In selecting a coal for smelting purposes, there are several circumstances to be attended to as well as the mere heating power—the sulphur present, which for some operations would render the coal useless—the ash and the composition of the ash, which should be such as to partially fuse or cake, for purposes to be afterwards noticed. Some of the Newcastle coals we have found equal, if not superior to the strongest Welsh coal, both in their heating quality and absence of sulphur; but owing to the small quantity of ash, and that not caking, but passing off as dust, they are by this cause unsuitable without the addition of matters to make up the defect.

All the conditions referred to are combined in the coal which the smelter designates strong coal, and one or more of the conditions are more or less absent in that of the weak coal. The following analyses of a few of these two qualities were made in hopes of finding how far a chemical examination would assist in determining the best quality of coal for smelting copper.

Strong coal.—Too strong to use alone for smelting, and requiring to be mixed with weaker coal from two different pits.

No. 1.

Coke . . 77·8
Volatilized 22·2 ... $\left\{\begin{array}{l}\text{Sulphur} \quad \cdot 8 \text{ per cent.}\\ \text{Water} \quad 1\cdot 9 \quad ...\end{array}\right.$
————
100·0

2·7

leaving 19·5 of carbonaceous volatile matters. Coke increased in bulk three times in caking.

Ash 3·1 per cent., which gave—

Silica	·4
Oxide of iron . .	2·0
Lime	·3
Magnesia . . .	·2
Loss	·2
	3·1

No. 2.

Coke . . 81
Volatilized 19 ... $\left\{\begin{array}{l}\text{Sulphur} \quad 1\cdot 8\\ \text{Water} \quad 1\cdot 0\end{array}\right.$
————
100

2·8

leaving 16·2 of carbonaceous volatile matters. The coke increased in bulk $3\frac{1}{2}$ times.

Ash 7·2 per cent., very red in colour, gave—

Silica	2·0
Oxide of iron . .	3·5
Lime	1·2
Magnesia . . .	·4
Loss	·1
	7·2

Strong coal.—Strong enough alone for smelting, but does not stand mixture with inferior qualities to work well or œconomically.

Coke . . 86
Volatilized 14 ... $\left\{\begin{array}{l}\text{Water} \quad 1\cdot 4\\ \text{Sulphur} \quad \cdot 5\end{array}\right.$
————
100

1·9

leaving 12·1 volatile carbonaceous matters. Coke increased in bulk three times in caking.

Ash 2·8 per cent., of a light red colour, gave—

Silica	·5
Oxide of iron . .	1·8
Lime	·4
Magnesia . . .	trace
	2·7

From another pit of similar quality of coal :—

$$\begin{array}{ll}
\text{Coke} & . \quad 85 \cdot 4 \\
\text{Volatilized} & 14 \cdot 6 \\
\hline
& 100 \cdot 0
\end{array} \quad \left\{ \begin{array}{ll} \text{Water} & . \quad 1 \cdot 8 \\ \text{Sulphur} & . \quad \cdot 4 \\ \hline & 2 \cdot 2 \end{array} \right.$$

leaving 12·4 per cent. of carbonaceous volatile matters.
Ash 2·6 per cent., gave—

$$\begin{array}{llll}
\text{Silica} & . & . & . & 1 \cdot 5 \\
\text{Oxide of iron} & . & . & \cdot 7 \\
\text{Lime} & . & . & . & \cdot 4 \\
\hline
& & & & 2 \cdot 6
\end{array}$$

Coke increased in bulk three times.

Weak coal.—Not strong enough to smelt alone, but used for mixing with strong coal, or for calcining, and other operations requiring less heat.

$$\begin{array}{ll}
\text{Coke} & . \quad . \quad 66 \\
\text{Volatilized} & 34 \\
\hline
& 100
\end{array} \quad \left\{ \begin{array}{ll} \text{Water} & . \quad 2 \cdot 5 \\ \text{Sulphur} & . \quad \cdot 9 \\ \hline & 3 \cdot 4 \end{array} \right.$$

leaving 30·6 carbonaceous volatile matters. Coke increased in bulk four times, very light and spongy.
Ash 2·8 per cent., gave—

$$\begin{array}{llll}
\text{Silica} & . & . & . & 1 \cdot 3 \\
\text{Oxide of iron} & . & . & \cdot 7 \\
\text{Lime} & . & . & . & \cdot 4 \\
\text{Magnesia} & . & . & . & \cdot 2 \\
\text{Loss} & . & . & . & \cdot 2 \\
\hline
& & & & 2 \cdot 8
\end{array}$$

From another pit :—

$$\begin{array}{ll}
\text{Coke} & . \quad . \quad 86 \cdot 7 \\
\text{Volatilized} & 13 \cdot 3 \\
\hline
& 100 \cdot 0
\end{array} \quad \left\{ \begin{array}{ll} \text{Water} & . \quad 2 \cdot 5 \\ \text{Sulphur} & . \quad \cdot 6 \\ \hline & 3 \cdot 1 \end{array} \right.$$

leaving 10·2 carbonaceous volatile matters. Coke increased $1\frac{1}{2}$ time in bulk in caking.
Ash 4·7 per cent., gave—

$$\begin{array}{llll}
\text{Silica} & . & . & . & 2 \cdot 2 \\
\text{Oxide of iron} & . & . & 1 \cdot 4 \\
\text{Lime} & . & . & . & \cdot 6 \\
\text{Magnesia} & . & . & . & \cdot 2 \\
\text{Loss} & . & . & . & \cdot 3 \\
\hline
& & & & 4 \cdot 7
\end{array}$$

These few analyses, from a great many, give the general cha-

racter of the qualities of coal used. The two last described as weak we have selected as being very distinct in their composition : the weakness of the first we believe to lie in the great amount of volatile matters carrying away the heat in their expansion ; that of the latter is its too close approximation to anthracite ; it does not cake or swell, but lies as solid as a stone upon the bars. We believe the physical character of the coke, when the volatile matters have passed off, is of great consequence. Taking the analysis apart from these other conditions, it would be difficult to say wherein lies the difference of quality. The following table shows the amount of carbonaceous matters only, both in the coke and the volatilized portion.

	Volatilized.	In coke.	Ash, water and sulphur.
Weak	10·2	82·0	7·8
Weak	30·6	63·2	6·2
Strong	12·4	82·8	4·8
Strong	12·1	83·2	4·7
Too strong ...	16·2	73·8	10·0
Too strong ...	19·5	74·7	5·8

From this table we observe a distinction in the composition of the different qualities ; a distinction, too, which a great many analyses have borne out in respect to strong coal—that the carbon in the volatile matter should amount to from one-fourth to one-fifth of the carbon in the coke. The great quantity of ash, and of iron in the ash, of one of those termed too strong, causes a fusion of the clinker, and consequently a quantity of fused glass to pass through the grate bars, making what the workmen term *a dirty coal*, and increases their labour to keep the bars clean, or rather the fire open, but is of no further disadvantage.

Some of the workmen have a practice of keeping the coal they are using wet, for the purpose of making it cake better, and it is supposed to give greater heat ; some throw water into the ash-pit to produce steam, which passes with the air through the fire. These practices are no doubt the result of experienced advantages. It is well known that if water be brought into contact with red-hot charcoal or coke, or steam passed over these at a red heat, carbonic oxide is formed and hydrogen liberated; which reaction is probably the reason why water or steam passed into a fire improves it for a short time for the reverberatory furnace. Plans have been tried, and we believe with success, to give anthracite coal some of the properties of strong burning coal by admitting a gentle current of steam to the fire ; but although the addition of water to coals, or steam to a furnace, may improve a fire or certain kinds of coal, still water will never impart or make up

for all the good properties which coals have that possess all the necessary elements within themselves; there is not the change of character, the caking and swelling, which is, we think, essential to the production of a continued high heat in an air-furnace.

If coal be allowed to dry or be exposed for a long time to the air, it soon loses its strength, and the subsequent addition of water will not restore the loss. Coals become greatly deteriorated during a long voyage owing to this cause. The best Swansea coal, in its voyage to Chili or Australia, loses nearly 12 per cent. of its value for smelting; a serious drawback to the smelting operations abroad, as the coal being weaker is less able to burn either alone or with wood. This is particularly the case with some of the South Wales coals, which are known to lose their strength by a few weeks' exposure. It is also known that such coal put into a ship's hold gives out fire-damp, so that it is necessary to ventilate the ship; showing that hydrocarbons are being evolved.

The copper smelters have a peculiarity in the management of their furnaces on which much of the art depends, and is quite consistent with the general laws of heat. The peculiarity is in making the clinker serve the purpose of fire-bars. In lighting a furnace, iron bars are used for the grate, having upon them some old clinkers or pieces of brick; the fire is continued until a new clinker is formed from the ash of the coal, which should be sufficient to cake by the heat of the fire, but not to fuse. As this clinker accumulates and enlarges, the iron bars that were used as a grate are removed; so that when the fire is in a good condition to smelt by, the whole bottom of the furnace is composed of a few immense clinkers supported by only two or three large bars of iron fixed in such a position as will bear them. From time to time they are changed as the fireman sees fit. These clinkers will often be from 1 to 2 feet thick, and weigh upwards of 1 cwt.; it is on this account that attention has to be paid to the nature of the ash in coal.

In the management of these clinkers for regulating the draught lies the secret of œconomical smelting, and it can only be attained by long experience and constant attention on the part of the workmen. Some workmen can do more work with less fuel than others, by a proper regulation of the draught passages, which are made and kept open by means of iron bars; breaking off pieces of the clinkers in one part, closing up large holes in another, and so on as the necessity and skill of the workman suggest.

The composition of the clinker is very regular, and may be termed a silicate of iron. We have submitted a good many of them to analysis; the average composition may be stated in

round numbers as under :—

Silica	61
Per- and protoxide of iron	27
Alumina	5
Lime	4
Magnesia	1
Sulphur	1
Carbon	1
	100

Le Play gives the composition of a clinker as—

Silica	52
Peroxide of iron	5·2
Protoxide of iron	22·0
Alumina	14·2
Lime	2·4
Magnesia	·7
Sulphur	1·3
Iron	1·0
Carbon	1·2
	100·0

With coals which give but a small quantity of a white and slaty ash, as some of the Newcastle varieties, a little ground slag or oxide of iron put into the fire will assist the formation of a proper clinker.

The advantages of this method of keeping the fire are obvious. The clinkers are bad conductors, which, with their thickness from the lowest part to where the fire is active, and the current of air passing up through them, prevents the heat from being radiated into the ash-pit. Moreover the air in passing through this mass of clinker gets heated, an advantage of no little importance in large fires.

The clinker being thus brought nearly level with the bridge, and the fire reaching to the roof, it will not burn upwards, but horizontally; so that when the furnace is in operation, it may be looked upon as a blast or cupola laid upon its side to prevent the materials mixing with the fuel.

It will be seen that the materials placed in the bed of the furnace to be smelted are not only subjected to a high heat, but are exposed to the action of the gas or gases given off by the fuel; and thus the nature of these gases and their power of acting upon the stuff are important considerations in the operation. The quantity of air admitted through the fire will modify materially the quantity and quality of these gases, and for some operations effect certain deleterious reactions in the materials to be smelted.

Should, for instance, the copper be in a certain stage of manufacture, and the coal contain much sulphur, the copper will be deteriorated if too little air be passing ; the sulphur will be merely sublimed from the coal, and will combine readily with the copper. Should there be sufficient air, the sulphur will be burned or converted into sulphurous acid, and the effects upon the copper will not prove so deleterious. The same with the other ingredients of the coal ; the carbon may either pass off as carbonic oxide or carbonic acid, and the action of these in the fusing chamber are different, and dependent upon the quantity of air allowed to circulate through the fire. Every 6 lbs. of carbon requires 8 lbs. of oxygen, equal to about 38 lbs. air, or 460 cubic feet, to form carbonic oxide ; and it will require double these quantities of oxygen or air to form carbonic acid. In the combination of 6 lbs. carbon with oxygen, only a definite quantity of heat is given out, which can be rendered less or more effective according to the time taken to combine with the materials. Thus if too little air be admitted into the fire, the combustion is slow, and a ton of coal may be consumed without effecting the fusion of the materials. The quantity of air, again, may be unnecessarily great ; and the heat so intense, as not only to fuse the substances in the chamber, but also the materials of the furnace ; so that the proper medium between these conditions is the object of a good workman.

Thus, it will be observed, a limit is put to the rapidity of the operations ; were it not for this, the more rapid the fusion and the stronger the coal, the better the smelted product. But the material to be fused forms a solid mass upon the bed of the furnace from six to twelve inches deep, and being a bad conductor of heat it is transmitted through it slowly ; consequently a certain time is required to fuse the whole, however intense the heat applied may be ; but this intensity has to be limited by the tendency which the bricks used for lining the furnace have for combining with the materials of the charge, and being thus destroyed. It is easier and more œconomical to use a mixture of strong and weak coal than to employ all strong and keep the combustion low by regulating the draught ; and the superiority of a workman often consists in being able to use a greater quantity of weak coal. The qualification of a workman is comprised in the following : to do the greatest quantity of work with the least quantity of fuel, having in it the greatest quantity of inferior fuel, and requiring the least amount of repairs upon his furnace. Indeed it would be worth the consideration of employers to grant small premiums to the men who excelled in these qualifications. We have seen a furnace burnt down roof and sides in twenty-four hours by a change of fuel and inattention on the part of the workman.

Our remarks apply particularly to fusing-furnaces. There are other furnaces for calcination where a lower heat is required, and where currents of air are admitted to the chamber otherwise than through the fire, producing actions and reactions within the chamber differing from those in the fusing-furnace, which will be noticed when describing the process of calcination; but the fuel and the principles of firing are the same, except that the weakest free burning coal may be used. For refining the metals generally, the best coal alone is used, and that known to contain no sulphur.

A fusing-furnace in constant work and of the ordinary size, consumes about three tons of fuel in twenty-four hours, but the consumption of fuel varies with the size of the furnaces; a fair average may be given at two tons of coal for five tons of stuff melted.

Patent fuel, which is culm or small coal mixed with pitch or tar and made into a cake by pressure, then baked at a high heat, has been tried, but does not compete either in price or quality with coal in this country; but not being subject to deterioration in a ship's hold, it is found to come into competition with coal exported from this country.

Many trials have been made both at home and abroad with a mixture of coal and wood, but they have not proved favourable to its use. Coal and wood, and patent fuel and wood, have been extensively tried abroad against coal alone and patent fuel alone, in their effective powers for fusing in reverberatory furnaces, but both coal and patent fuel alone are far superior to any of them mixed with wood; of course the œconomy of one over the other is a question depending upon the locality and the price of each of the materials; we speak only of their effects in smelting by reverberatory furnaces. In smelting copper by blast or cupola furnaces, such fuels as anthracite, charcoal, &c., which cannot be used for the reverberatory furnace, are best. Copper smelting has been carried on in Australia these few years by blast furnaces, using for fuel a mixture of coal and charcoal. In Austria, Hungary, and several other parts of the Continent, all the copper is made by means of blast-furnaces, the fuel being charcoal: these operations, and the kind of furnaces used, will be considered in their proper place.

In some parts smelting operations are carried on by means of peat-charcoal; a fuel, we think, well suited for this purpose when accompanied by blast. With such a field as Ireland, with plenty of copper ore and peat within herself, the operations of smelting could be done easily and profitably. Irish ores are well fitted to be treated by the blast, and they give a superior quality of copper.

[To be continued.]

XLIII. *Pendulum Experiments.* *By* Thomas·G. Bunt.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE lately been repeating M. Foucault's experiment in my own house, and endeavouring to ascertain with what degree of accuracy it may be performed on a comparatively small scale, and at a much less expense of time and labour than I had bestowed on it on a former occasion. I have accordingly suspended, in different parts of my house, three different pendulums, and now send you the results of the experiments I have made with each of them.

My longest and heaviest pendulum was erected in my staircase, where, by making an aperture in a landing-place, I obtained a length of 19 feet. Here I suspended from a strong piece of timber fixed across the angle of two walls, the leaden ball of $53\frac{1}{2}$ lbs. weight, with which I experimented last summer in the spire of St. Nicholas, and enclosed it in a glazed case, (having a small aperture at the top for the wire,) in order to protect it from currents of air. The azimuth circle, 7 inches in diameter, was furnished with vertical and horizontal adjusting screws, and usually brought within about $\frac{1}{20}$th of an inch below the point of the wire projecting from the ball. Parallel lines, $\frac{1}{20}$th of an inch asunder, were drawn across the circle, by means of which the ellipticity, or semiaxis minor (*b*), was read off and recorded, in each experiment, to the $\frac{1}{200}$th of an inch. The advantages of short arcs (which I first perceived and pointed out last summer) have now become more than ever apparent; so that in the greater part of my recent experiments I have seldom drawn my pendulum more than 1 inch from the perpendicular. Commencing with a semiarc of this magnitude, my 19-feet pendulum will keep up its vibrations, so that they may be seen, and their azimuthal direction ascertained within 3 or 4 degrees, after a period of 35 or 40 hours. I have not, however, introduced into my present series any experiments of long period, but confined myself to those of which the mean ellipticity has been observed and recorded.

The centre of suspension of this pendulum is similar to that which I employed at St. Nicholas, and described in my letter contained in your Number for June 1851; with this difference only, that a hole is drilled through the axis of the divided screw, into which is inserted a steel cylinder composed of four segments like the screw itself, up which the wire passes, and is thus compressed between four pieces of hardened and tempered steel instead of brass, which had been found too soft to resist the

continued action of the wire. This centre appears to be better, and to produce less ellipticity of motion than any other that I have tried. As a specimen of its performance, I copy from my minutes the concluding experiments of this series, which are as follows :—

	Azimuth.	Semiaxis major (a).	Semiaxis minor (b).	
h m Started at 8 24 A.M. 10 59 0 32 P.M. 1 45 4 30 6 43	25·3 55·9 73·5 88·0 ...... 150·0	in. 0·60 0·47 0·37 0·30 0·25 0·20	in. 0·00 0·00 —0·01 —0·03 —0·03 —0·01	One impulse only.

Thus vibrating 10^h 19^m without more than $\frac{1}{33}$rd of an inch ellipticity, its mean hourly motion in azimuth being $12°·09$.

The following is a summary of the whole series of experiments collected into two groups, according to the direction of the elliptic motion. When the sign of b is positive, the motion is from N. to E.; when negative, from N. to W.

Total time.	Total motion in azimuth.	Mean value of b.	Mean rate per hour.	
h m 60 54·6 61 11·4	722·9 716·0	in. +0·0155 —0·0160	11·87 11·70	Mean value of a about 0·5 in.
122 6	1438·9 = 11°·784 per hour, mean of the whole.			

The mean hourly motions for every $20°$ of the azimuth are as follows :—

8 198	38 218	58 238	78 258	98 278	118 298	138 318	158 338	178 358	Azimuth. Azimuth.
11·81	11·62	11·44	11·79	11·86	11·67	11·91	11·67	11·93	Per hour.

My second pendulum was 8 feet 10 inches long, with a ball of lead weighing 35 lbs., and somewhat differently suspended. The wire was fixed in a hole drilled through a bar of iron, which was screwed to the floor. A saw-curf was carried from one end of the bar, through the centre of the hole, and a little beyond it; and the two portions of the bar firmly pressed against the wire by a vice. It answered its purpose very well, and produced but little ellipticity.

The results obtained from this pendulum were as follows :—

Total time.	Motion in azimuth.	Mean value of b.	Mean rate per hour.	
h m 36 52·6 35 34·7	419·5 433·4	in. −0·0250 +0·0275	11·38 12·18	Mean value of a about 0·75 in.
72 27·3	852·9	+0·0012	11·773	
Deduct, to reduce mean value of b to 0... ·018				
Mean motion per hour from this series ... 11·755				

Mean Hourly Motions for every 20° *of Azimuth.*

15 195	35 215	55 235	75 255	95 275	115 295	135 315	155 335	175 355	Azimuth. Azimuth.
11·79	12·24	12·27	11·27	12·74	12·92	11·64	11·17	10·87	Per hour.

My shortest and lightest pendulum had a length of 8 feet 2 inches, and a ball of $4\frac{1}{4}$ lbs. The mean arc of vibration given to this pendulum was longer than in the two former cases, and the ellipticities greater. The experiments are accordingly collected into eight groups, according to the different values of b, and give the following results :—

$$b = +0·0125 \text{ inch.} \quad \text{Motion per hour} \quad 12·70$$
$$0·0375 \quad ... \quad ... \quad 13·22$$
$$0·0605 \quad ... \quad ... \quad 14·13$$
$$0·1050 \quad ... \quad ... \quad 14·63$$
$$b = -0·0155 \quad ... \quad ... \quad 10·73$$
$$0·0400 \quad ... \quad ... \quad 10·70$$
$$0·0635 \quad ... \quad ... \quad 8·79$$
$$0·0850 \quad ... \quad ... \quad 9·32$$

Mean value of a about 1·2 inch.

Mean of all.

Total time.	Total motion.	Motion per hour.
h m 54 49·6	643·9	11·744
Deduct to reduce $b = 0$		·066
Mean motion per hour from this series		11·678

Each of these three pendulums, like those with which my experiments were performed last year, had its two nodal lines of azimuths placed at right angles to one another, on arriving at

which the elliptic motion gradually changed from one direction to the opposite one. My 19-feet pendulum, when approaching one of these nodal lines, would frequently, as in the example above cited, vibrate for hours without any perceptible ellipticity.

The latitude of my house being about $51° 27'·8$, the mean azimuthal motion per hour, according to Foucault's theory, would be $11°·764$, which exceeds only by $0°·025$ the mean of these three experimental results.

It thus appears, that if sufficient care be taken in the construction of the centre of suspension, and the arc of vibration be confined within very narrow limits, this beautiful experiment may be successfully performed with a pendulum of very moderate dimensions.

I am, Gentlemen,
Yours very respectfully,
Thomas G Bunt.

Bristol, 7 Nugent Place,
September 13, 1852.

XLIV. *Report on* M. Pasteur's *Researches on Aspartic and Malic Acids. By* M. Biot[*].

THE investigation of which we are about to lay an account before the Academy is essentially appertaining to molecular chemistry. It is the examination of a case of isomerism the most extensive and intimate that has been yet observed, and it is accompanied by striking peculiarities of an entirely novel character. The phænomena of isomerism are in themselves among those which are most capable of affording an explanation of the mechanism of chemical reactions, by giving us an opportunity of investigating by comparison what are the molecular conditions which render these reactions so different in substances consisting of the same chemical elements united in the same proportions of weight. But these abstractions, which include the whole science, cannot be deduced from known facts except by a train of mechanical and physical reasoning; the first term of which commences with their most simple phænomena, and the last terminates with their most remote causes. We are therefore compelled in the present case to introduce the principal links of this logical chain, for the purpose of proving that the researches of M. Pasteur have added new elements to our knowledge of these phænomena. If the rapid exposition which we are about to give should appear in the first instance to be a digression from our proper aim, we plead as an excuse that we have ineffectually endeavoured to discover any other method by which it could be attained, consist-

ently with the time which is allowed for the treatment of the subject, and without sacrificing that precision of argument which the subject demands.

The first step towards the solution of a scientific problem is to state it with perspicuity. We shall apply this principle to the one of which we are about to treat. According to the general conception of chemical phænomena, and it is necessary to take a speculative view of them to be able to arrange them in the form of a science, the substances between which they take place are considered as so many corpuscular systems of different natures, the constituent molecules of which are more or less complex. Some of them do not undergo any alteration under the influence of any operations to which they have hitherto been submitted. These belong to that class of substances which are called *simple*. Others, on the contrary, and they constitute the most numerous class, are capable of being separated by chemical processes into molecular groups of a less complex order, which in their turn are ultimately resolvable into molecules which belong to the class of simple substances. Such molecules as are chemically separable constitute the substances which are called *compound*.

In all these systems the constituent corpuscles are individually imperceptible to the senses in consequence of their minuteness. Nevertheless, with this extreme minuteness, they are considered to possess all the properties of tangible matter. Thus they are assumed to be extended, possessed of figure, and to be themselves composed of parts physically aggregated in various numbers. They are, in short, according to our ideas, so many distinct minute bodies, possessing, like the planets, an attractive force proportional to their mass and reciprocally as the square of their distances, which manifests itself in their weight when they are aggregated in large quantities; perhaps also they act upon each other at a distance in virtue of forces decreasing more rapidly, which they would exercise conjointly with the former, and which we must distinguish from the first-mentioned force by their apparent modes of action, although they might in reality be only complex derivatives of the same general law. The corpuscles thus defined preserve all their individual properties in the sensible masses formed by their aggregation. But conformably to the conceptions which general physics give us of the conditions under which these aggregates exist, the corpuscles are considered to be always maintained out of mutual contact, either by the repulsive forces which emanate from them, or by the interposition of a medium of inappreciable ponderability, which prevents their actual contact by opposing a resistance to them, or by repelling them.

These circumstances of condition are common to all the sub-

stances upon which chemical influence is exerted; they are the mechanical expression of their actual mode of existence, such as it presents itself to our observation. But the vital organism gives rise to a number of compounds, the parts of which chemically similar have an intimate, and, as it were, natural correlation with each other, which is due to their physiological mode of generation. These substances are called organized. The chemical corpuscles of which they are composed, being considered independently of any relative coordination, are called organic bodies, in allusion to their natural origin, without, however, attributing to their simple elements any other properties than those which they evince in the generality of combinations into which they enter. Can it be that the delicacy of the processes by which these invisible corpuscles are united, impresses upon them a peculiar interior organization? We are unable to answer this question. Hitherto the rotatory power of molecules has only been established in that class of substances elaborated by vital organisms.

Without being acquainted with the nature of the particular forces which emanate from the separate corpuscles of which each substance is composed, experience has proved that those which chiefly determine chemical actions exercise influences, the intensity of which decreases very rapidly when the distance is increased; for all the different phænomena of this kind take place within distances which are to us entirely inappreciable. These phænomena consist in the fact, that substances whose molecules mutually approach within these limits occasionally combine or separate into corpuscular systems differing from those existing before. These mechanical actions constitute what are called the chemical combinations and decompositions. Probably only an imperfect conception would be formed of these phænomena, by comparing them to the action of two celestial nebulæ which penetrated each other.

In spite of the excessive complication which this comparison itself assigns to them, it is necessary, at least in imagination, to distinguish two kinds of phænomena in these reactions, differing from each other in the mechanical conditions of their fulfillment. The former take place when the mutual distances of the corpuscles which react upon each other are so great in comparison to their actual dimensions, that all the elements of the mass of each corpuscle of a similar nature exercise actions whose intensities are virtually equal, whatever may be their relative situation in its interior. The other phænomena begin to take place when the mutual distances of the corpuscles have become sufficiently small, that the relative situations of the elements of their mass produce sensible inequalities in the absolute intensities of their individual

actions. The first class of phænomena would depend solely upon
the particular nature and the total mass of the different ingredients
contained in the corpuscles of each substance, as also upon the
special properties which might naturally appertain to each of
these taken as a whole. The latter class of phænomena would
depend, further, upon the position which each ingredient might
occupy in the corpuscle, upon their relative arrangement, and
upon the configuration of the entire corpuscle.

These two orders of effects of attractive forces are realized with
the greatest strictness in the motions of the planetary bodies
which compose our planetary system; and they can be easily
observed there. The general motions of revolution which the
planets execute in their orbits, and the occasional derangements
which they undergo, take place without any appreciable differ-
ence, as if their masses were individually concentrated into a
mathematical point coinciding with their centre of gravitation.
This, then, is the first order of phænomena. But the relative
situations of the elements of mass which compose the bulk of
each planet have a sensible and determinate influence upon the
oscillations of the fluids which cover them, and upon the various
motions which each executes round its centre of gravity, inde-
pendently of its constant rotation on its axis. This represents
the second order of phænomena. In a mathematical point of
view, the one and the other ought to take place with analogous
characters in all the systems of free bodies possessing reciprocal
actions which are exercised at a distance. But the effects which
belong to them may have relations entirely different from those
which we observe in our planetary system. Their phases of
simultaneous accomplishment may become so sudden and so in-
terwoven together, that observation, although knowing the fact
of their existence, is unable to detect them.

This is exactly what happens in chemical reactions; and it may
readily be conceived why this should be the case, when the me-
chanical conditions of the two classes of phænomena are com-
pared. The permanent bodies of our planetary system have all
a nearly spherical form. The intervals by which they are sepa-
rated always remain very great in comparison to their actual
dimensions. At those distances, the attraction proportional to
the masses and reciprocal to the square of the distances is the
sole force which has any appreciable influence upon their motions.
They move in a space virtually destitute of resistance; and their
masses remain constant, or at least, during the ages that they
have been observed, no alteration has taken place which could
be detected. Finally, their number is small; and their masses too
are all very small compared with that of the principal body round .
which they revolve. This concurrence of circumstances gives to

the problem of celestial motions all the simplicity which can be consistent with its nature.

In the case of chemical phænomena, on the contrary, the mechanical conditions of the motions and even their phases are hidden from us. We are ignorant of the form and intimate constitution of the corpuscles which react upon each other. These themselves, as well as the intervals which separate them, escape the observation of our senses; so that we are unable to ascertain both the relations between their dimensions and their mutual distances, or in what proportions the latter vary. The particular forces which each corpuscle exercises within these inappreciable limits of variation are unknown to us. The only character which we are able to attribute to them is, that they decrease with such rapidity when the distance is augmented that they become inefficacious at all distances sensible to us. Moreover these forces alone do not determine the phænomena, or at least their influence is not absolute; for we invariably see their effects modified by the intervention of imponderable principles, which we employ as agents without knowing in what their nature consists, or in what manner they contribute to the production of the observed results. Lastly, to increase the complication, the actions thus exercised are so powerful, that the respective masses of the corpuscles undergo a convulsive change which resolves them into less complicated groups, or causes them to aggregate into new ones. These convulsive alterations represent to us those which, in proportions vastly magnified, would take place in the fluids which cover our terrestrial spheroid, if the cosmical bodies which cause their successive rise and fall were to approach sufficiently near to its solid nucleus to remove them wholly or in part from its influence.

In this absence of direct data to set about the solution of a problem so complicated, modern chemistry—to its honour be it said—has not remained a pure science of facts. In proportion as its progress has brought to light an enormous number of facts, it became necessary to systematize them in accordance with their most apparent relations. This labour of coordination has brought to light those empirical laws, which, within the sphere of application which each one embraces, enable us to foretell almost infallibly all the analogous results which ought to be produced, if not extending to all their details, at least in all the general circumstances of their fulfillment. From these all the inductions have been drawn, which, in the majority of cases, proves with great probability what mode of decomposition, of recomposition, or of mutual replacement, must have mechanically taken place in the substances placed in contact, and what systems of corpuscular groups have been definitely disunited or formed in their

reaction. Then mentally tracing back the actions of those groups to the invisible corpuscles which compose them, it has become possible, justly and without hypothesis, to characterize these individually in each substance by the triple conjunction of their observed properties, with the nature and the relative proportion of the ponderable ingredients of which they are composed.

The science of chemistry has arrived at these abstract principles which appear to be unattainable in two distinct ways, that of coordination and that of speculation. In the former of these, chemistry relied solely upon its own capabilities, and did not extend its contemplations beyond the immediate results of experiment. Thus was analysis brought to a state of perfection, the law of multiple proportions discovered, and the calculation of equivalents invented. This calculation has been to chemistry the principle of all generalization. For in the first place, by defining the results of analyses no longer according to their numerical details, which leave them isolated, but by the relative masses of the different simple ingredients which constitute each substance, it has rendered evident one of the principal mechanical conditions of their individual existence which it has been possible to express generally by means of a literal notation of extreme simplicity. Then as the law of multiple proportions was naturally realized in those expressions, all the substances analysed have been symbolically represented by the association of two characters; the one specifying the particular nature of each ingredient, the other stating the multiple resulting from its conventional unity which enters into each substance under consideration.

These two characters comprise all the data which direct analysis is capable of affording. But after the exposition which we have given of the chemical problem, their reunion would be insufficient to establish a complete definition; for they do not define the relations of mass which there may be between the corpuscles constituting different substances; they do not express anything which may have a relation to the particular configurations of those corpuscles; nor further, what is the internal distribution of the different ingredients of which they are composed. Now these are so many determining peculiarities of the actions exercised by the corpuscles. Thus a number of substances have been met with, which, although consisting of the same elementary bodies united in the same proportions, possess physical and chemical properties very different. These substances are called isomeric. It was from that time necessary to search elsewhere than in the results of direct analysis for general characters of identity or of difference, which might be annexed to the symbolic

formula as a complement of the definition in that case as well as in all others.

This problem becomes solvible by a very natural analogy, when the substances under consideration, being placed in similar circumstances, form combinations with other substances, the products of which are constant and clearly defined, and do not differ in their composition except with regard to the relative masses of dissimilar matter which are respectively associated to a given weight of the isomeric substances which are compared together. The equivalents of weight, and consequently the specific masses of the corpuscles, are then proportioned to these relative quantities, which process gives the common factor by which their symbolic formulæ are to be multiplied in order to retain them constantly in this proportion.

It is thus, for instance, that the four isomeric compounds of cyanogen with oxygen, which are called cyanic and cyanuric acids, cyamelide, a neutral substance, and fulminic acid, are characterized by a distinct factor. The difference in the nomenclature thus applied to these four bodies is evidently unassailable when it is employed as a symbolic representation of the observed facts. But its physical interpretation implies an inference, the accuracy of which is only probable. It is, that in those experiments the unequal composition of the products investigated should be solely attributed and proportionate to the relative masses of the isomeric corpuscles, the ingredients of which are combined therein. Thus in such cases, also, chemists avail themselves of every analogy calculated to confirm the proportionality which they assume.

The study of organic substances, which has attained such a wide extent, presents a great number of instances of isomerism, which are far more difficult to define even symbolically than those above mentioned, because the reactions which take place between them and other substances almost always transform those which are to be tested, and then only characterize them indirectly by means of their products of decomposition, and, so to speak, only after they have ceased to exist, which tends to extinguish the original characters of dissimilarity, which would be a key to the isomerism.

In order to escape from these ambiguities, chemists have endeavoured, and successfully, to find among those sciences which are connected with chemistry, auxiliary evidence, the application of which would be exempt from any destructive influence. From physics has been derived the law of volumes, while the determination of the density of bodies in the state of vapour, as well volatile as non-volatile, and the estimation of their specific heats, have afforded means of distinguishing different substances by a new class of equivalents in addition to the equivalents of

weight.   Crystallography has pointed out the conditions of iso-morphism; and it is certainly a circumstance no less curious than instructive, to observe how a science which is concerned solely with the study of form, has been able to render assistance to that which is exclusively occupied with that of molecular phænomena. But the separation which has been made between these two branches of investigation, in order to facilitate their comprehension, is merely an artificial one, and they are really very intimately connected.   The word isomorphism, in the sense in which it is employed by chemists, expresses a triple analogy of properties, which are very frequently, though not invariably, presented by substances having the same chemical formula, that is to say, those in which the sole distinctive feature consists in the different nature of their constituents, simple or complex, expressed by their literal symbols.   Among those substances having similar formulæ, a large number spontaneously aggregate when they crystallize separately into geometrical solids, the forms of which are identical with each other, or vary very slightly.   They also crystallize conjointly by their united action when they are dissolved together in all manner of proportions; and then if the operation is well conducted, the crystals which are formed possess a continuous structure and a homogeneous composition throughout their entire mass, still presenting the same relations of form with each other and the preceding crystals, except some trifling inequalities in their angles; as if the corpuscles of different kinds which constitute the mixtures, when they aggregate in crystalline groups identical with each other and of heterogeneous composition, were brought by their mutual reaction to take relative positions, differing very slightly from those which they would have done when arranging themselves separately.   It is not difficult to conceive in what manner the analogies so closely dependent upon the molecular forces can have been of service to the science of chemistry.   It is these analogies which have afforded to it the means of explaining and reducing to the comprehensive law of definite combinations by simple multiples an immense number of natural and artificial products, the complex and irregular composition of which appeared to constitute an important exception to that law.   Moreover, inasmuch as compound bodies must, according to this principle, be analogically related or opposite in character according as they may be isomorphous or not with each other, very strong arguments have hence been deduced for the purpose of distinguishing, by comparative examination, those bodies which ought to be arranged in the same order or in different orders of atomic combinations; which has furnished theory with a new groundwork and novel conditions of coordination.   Finally, as always happens by the alliance of sciences, the

principle of isomorphism has not only benefited chemistry, but mineralogy has also received a fresh impulse from this discovery ; for it then became possible in that science to form a conception of, and define by precise formulæ, the abstract types of many mineral species which scarcely ever occur naturally in a state of purity, because they are constantly met with mixed with substances isomorphous with their principal elements, which can and, indeed, must necessarily, exist with them in proportions more or less abundant when the combination took place. Thus, in an analogous case, geometricians have a perfect idea of a circle, although neither nature nor art ever presents such a thing as an absolutely perfect circle.

In proportion as the science of chemistry advances in the investigation of the intimate constitution of bodies, wherein lies its future development, it cannot do other than gain by contact with those sciences which study the same objects under a different point of view, and by processes differing from those employed by it. Two sciences especially, crystallography and optics, appear to be destined to be henceforth its auxiliaries, which are not merely useful, but absolutely indispensable, in order to support and legitimize the theories which are suggested by the results of chemical investigation. It is however true that the former will not furnish any characteristics which can be applied directly to the corpuscles between which chemical actions take place. According to all appearance, the minute solids of similar nature, by the aggregation of which every crystal of sensible dimensions is formed, are numerous assemblages of those corpuscles, which, by their reciprocal attractions, are grouped together among each other according to a certain mode of relative arrangement in the physical conditions in which they may be placed. The crystalline form which is observed in the mass would therefore be a joint consequence of those attractions, and the circumstances which modify them. Thus, by varying these circumstances, and following with attention the peculiarities which under their divers influences present themselves both in the whole and the details of form, indications will be discovered which will have a connexion more or less intimate with the attractive forces exercised by the chemical corpuscles, the assemblage of which constitutes the embryo crystal. These general studies might be considerably facilitated and rendered more positive by the observation of the power of rotation of polarized light, which in the numerous instances in which it exists makes us acquainted with specific properties inherent in the chemical corpuscles themselves, not only after the trials which may have caused some modification in them, but by the mere observation of the sensible effects which are produced upon

polarized light in the actual state in which they are observed. Up to the present time, this power is, next to that of gravitation, the only observable character which it is possible to apply to them individually.   There is therefore a strong reason for adhering to it in the first instance, and to prefer the investigation of those combinations in which its presence is capable of serving as a guide.   Now these are at once the most diversified, and those whose constitution it is most difficult to explain decisively from merely chemical indications, by reason of their instability, joined to the feebleness and frequent indistinctness of their reactions.

The novel and valuable series of researches which M. Pasteur has pursued during the last four years with a success worthy of his perseverance, confirms all the arguments which we have just stated.   These researches have been conducted so as to unite the appliances of crystallography, chemistry, and molecular optics. In these reside both the principle and the condition of success. In fact, by the suppression of one term of that conjunction, no matter which, the two others, either united or separately, could not have done more than furnish to the most sagacious minds isolated and scattered results, between which that connexion, which at the present time constitutes their principal value, would be entirely overlooked, and each of which would be of no further importance than as being a fact of detail added to those previously known.   But by this happy union of all the experimental methods, by means of which the field of investigation upon which M. Pasteur has entered could be explored, the whole series of phænomena which he studied revealed themselves to him.   Not only has the racemic acid, which was hitherto supposed to be an homogeneous substance, been separated into two other acids molecularly different, possessing equal and contrary powers of rotation ; but further, the crystallographic characters, by which those compounds are distinguished from the neutral system which their combination affords, have been investigated, traced and established, not only in the bodies themselves, but also throughout the whole range of their crystallizable salts. These characters have been subsequently detected in numerous other organic products, which possess or are destitute of the power of rotation ; they have become the indications, not yet universally, but very frequently presented by these two states. Thus has been seen for the first time the manifestation of observable relations between the natural properties of the imperceptible molecules of which bodies are composed, and the configuration of the sensible masses which results from the aggregation of these molecules in crystals.

The recent memoir which M. Pasteur has just submitted to your notice, and of which we are now about to render an account,

has been conducted with the same conjunction of acquaintance with different branches of science and experimental methods as the former. The only difference is, that in this instance a circumstance which had not been foreseen furnished him with the principal subject matter; so that it might be said that it was met with by chance, if indeed the name of chance may be applied to the following out of a phænomenon recently announced as an isolated fact, of which a mind prepared to receive it at once recognises its important relation to his previous investigations, and developes the general consequences which result from it. The circumstances which have led to that development in this particular instance are worthy of being recalled, for they clearly confirm the thesis which we have endeavoured to establish at the commencement of this report.

Last year M. Pasteur made his researches upon asparagine, aspartic acid and malic acid. These two acids are theoretically and practically derived from asparagine, by taking from it one or two equivalents of ammonia. It was precisely for this reason that he selected these three bodies as the subjects of his study. The molecule of asparagine possesses the power of rotation. He wished to ascertain if that power was preserved after the progressive abstraction of the elements of ammonia; and if such was the case, what modifications it underwent. Experiment proved that it was retained, and that it ceased only when the transition from malic acid to those pyrogenous acids called maleic and paramaleic took place; the latter of which is also called fumaric acid, in consequence of its being met with in the common fumitory. This brief *résumé* is sufficient for our purpose. Whilst M. Pasteur presented his memoir to the Academy, M. Dessaignes of Vendôme announced that he had succeeded in preparing aspartic acid artificially by acting upon the acid fumarate of ammonia by a process which he described[*].

This announcement immediately presented to M. Pasteur an alternative, the experimental examination of which could not fail to lead to an important discovery,—either that the aspartic

* M. Dessaignes had previously obtained artificial aspartic acid by acting upon the bimalate of ammonia. He communicated this fact in a note which was presented to the Academy at the séance of the 18th of March 1850, and which is inserted in the *Comptes Rendus*, vol. xxx. p. 324. But in a subsequent note, also presented to the Academy upon the 18th of September in the same year, and inserted in the *Comptes Rendus*, vol. xxxi. p. 432, M. Dessaignes added, that he had reproduced the same artificial aspartic acid as a derivative of malate and fumarate of ammonia by the same process. This second announcement surprised M. Pasteur, because these two salts, which are designated as generators, do not possess any rotatory power. He has, moreover, established the fact by numerous repetitions of the experiment, that the second mode of derivation indicated by M. Dessaignes is not less accurate than the former, and virtually leads to the same result.

acid derived from the fumarate possessed, like the natural acid, a rotatory power, in which case it would be the first instance of an active body which could be derived from an inactive body; or the artificial aspartic acid would be inactive, and in that case, in spite of the identity of its chemical composition, it would be molecularly different from the natural acid. The latter conjecture was found to correspond with the fact. M. Pasteur established its correctness directly upon the small quantity of the artificial acid which M. Dessaignes was so obliging as to provide him with, after he had informed him of the unexpected interest which was attached to it. He also recognised in several of the small crystals a difference of form from that of the natural acid. The agreement of these two characters established the distinct nature of the new product; and the ingenious chemist who obtained this body doubled his right to the gratitude of science by evincing such generosity as to communicate it, that it might be investigated from a point of view apart from his especial studies. But these first observations only contributed to point out a fertile subject of investigation to any one who should follow it up in all its details with patient industry. M. Pasteur has devoted a whole year to effecting this object, and the results which he has obtained are collected in the memoir which he has just submitted to the public. We cannot do more than give a brief summary of it.

He submitted the two aspartic acids, the active and the inactive, to all the trials, both physical and chemical, by which the identity or difference of their molecular constitution could manifest itself. They were first examined in their uncombined state: he determined comparatively their elementary composition, their respective crystalline forms, their density and solubility in the same solvents. He then combined them with bases, and determined the composition of their respective salts, which he found constantly identical in pairs, with the peculiarities of difference of form, which he carefully estimated. He finally traced them through their chemical derivatives; and by subjecting them to parallel modifications of treatment, he obtained from them two malic acids, the one possessing and the other destitute of rotatory power, like the bodies from which they were produced. He then submitted these two products to the same series of tests which he had applied to their generators, that is to say, he determined in a parallel manner their crystallographic, physical, and chemical characters, as well in combination as in a free state. It is in this manner that the unexpected problem which was presented to him has been studied in all its branches, all the particulars which it was capable of affording, and by all the processes of observation and experiment which could be applied to it.

Three general classes of facts have resulted from this mode of treatment belonging to the three points of view under which the subject was considered. We shall class them in his own manner, indicating for each the proofs on which it rests, and the consequences deducible from them.

1. *Optical examination.*—The power of rotation which the natural aspartic acid possesses is communicated to all its salts, to the malic acid which is derived from it, and to all the salts of the latter acid. It disappears in the pyrogenous acids.

This power is null in the artificial aspartic acid, in all the salts of it which have been prepared, in the malic acid derived from it, and all its salts; neither does it exist in the ulterior pyrogenous acids derived from it.

For the sake of brevity, we shall designate these two classes of bodies by the denominations of the active series and the inactive series. The possession or the privation of the powers of rotation which distinguishes between them, proves that, in the corresponding terms of the two series, salts or acids, the constitution of their chemical molecules differs, since the members of the one class produce individually an observable influence upon polarized light, which those of the other class do not produce.

2. *Crystallographic examination.*—The corresponding bodies of the active and the inactive series, when dissolved in the same medium and placed in parallel circumstances, generally afford crystals differing in form, sometimes very slightly, sometimes altogether incompatible. The fact of incompatibility may without doubt be assumed to be owing to accidental circumstances of dimorphism. But the constancy with which they are reproduced within certain correspondent terms of the two series, when all the circumstances are parallel, joined to the invariability of the differences which this same identity of circumstances induces in the other case, suffices, independently of all explanation, to prove that the constituent molecules of the corresponding bodies which are compared together must be differently constituted in the two series, in conformity to the proposition which the difference of their optical properties had already established.

3. *Chemical examination.*—The elementary composition of the corresponding bodies is identical in the two series. Their molecules consist of the same ponderable principles combined in the same atomic proportions. Every operation, which, applied to any one of them, causes fusion, solution, decomposition, or combination with other substances, produces similar effects upon its correspondent of the other series, and yields products, the composition of which is identical. But while supposing the operation to be always effected comparatively and in parallel circumstances, there are generally differences observed

in the details of its action and in its effects.   Such, for example, are the differences, frequently slight, but nevertheless constant and appreciable, in the fusibility, the solubility, or the time necessary for the accomplishment of certain transformations, as if there existed between the molecules of the bodies compared a relative aptitude of greater or less extent to assume simultaneously this or that state.   Thus when crystals of active and inactive malic acids, which are completely isomeric, are placed together in a moist atmosphere, the crystals of the inactive acid absorb during two or three hours the very small quantity of water which they are capable of taking up, after which their weight does not suffer any further change.   The active crystals, on the contrary, absorb water slowly and progressively until they are converted into a viscous liquid.   The compounds of active and inactive aspartic acid with hydrochloric acid present the same difference in a more decisive manner.   The active and inactive malates of lead, when precipitated from their respective solutions, are amorphous, and after a certain time both assume the form of acicular crystals.   But in circumstances in every respect parallel, the time which the active malate requires for this change is frequently only a few hours, while for the inactive malate it is frequently several days.   All the correspondent products of these two series evince similar differences in what may be called their individual disposition.   Discrepancies of this nature are considered as unimportant in the ordinary course of chemical operations ; and there is perhaps frequently sufficient reason for their being disregarded as being dependent upon physical accidents unconnected with the molecular constitution, and as being too minute to be considered as essential characters.   But in the two series of bodies, active and inactive, which M. Pasteur has studied, these feeble inequalities acquire an entirely different importance ; for they there become the sensible indications, and the natural consequences of the difference in the molecular constitution of the corresponding bodies of those two series which had already been made known by the optical and crystallographical tests.

M. Pasteur justly remarks, that an example of isomerism is here presented as complicated as, and at the same time more consistent, more extended than, any which have hitherto been observed in chemistry, great as is their number.   The constant identity of the effects which the same reactions produce on the corresponding terms of the two series, affords him strong reasons for presuming, that if it were possible to produce active asparagine from active aspartic acid in a similar but inverse manner to that in which the acid is derived from the asparagine, a parallel process applied to the inactive aspartic acid would give an inactive asparagine also isomeric with the natural.   The same analogical

reasoning, strengthened by other examples already known, appears to him to permit the conjecture that many organic substances naturally endowed with the rotatory power may have in the same manner their inactive isomeric equivalents, which chemists should endeavour to obtain. Thus also, inversely, an organic product obtained artificially can no longer be considered as identical with the natural substance which its composition and even its reactions represent, unless the identity, or at least the equivalence of the crystalline forms, have been established, and above all, the presence of the rotatory power when the substance which is to be reproduced possesses it naturally*.

Let us for a moment consider these same facts of isomerism in a purely chemical point of view, by deducting all the data which their crystallographical and optical study have contributed towards their explanation. Let us suppose that the two aspartic and malic acids, together with their respective salts, had been occasionally obtained without their distinctive molecular characters having been known. In this case there would be two series of bodies, the correspondent pairs of which would have shown themselves as identical with each other in composition, reactions, atomic formulæ, and derivative products. Thus, according to principles, or as we ought rather to say, according to the practice habitually adopted by chemists, these facts would inevitably lead to the confounding of these two classes into one. This was in fact what happened when artificial aspartic acid was originally discovered, and it could not have been otherwise. Nevertheless, on examining them more closely, differences are perceived, slight, it is true, but constant and appreciable, in the facility with which the various transformations of the bodies take place, in the time which they take to act, in the temperatures and the quantities of the same solvents which are necessary to produce parallel results. Now these peculiarities, which are ordinarily neglected, are found here to be intimately related to those molecular differences which their existence should have sufficed to point out. It is these differences which we now discover to be in themselves far more important than there has hitherto been occasion to believe, and to which it is necessary in general to devote much more attention than it has been customary to do. The same change is effected in chemistry by this discovery as was formerly

* The rotatory power furnishes a strong, although not absolute indication of identity when it is found the same in direction, in absolute intensity, and the mode of dispersion in equal quantities in parallel solvents taken at the same temperature. The absence of one of these conditions is a positive proof of difference. The identification of the crystalline forms may sometimes be rendered uncertain by the accidents of dimorphism. It is this case of possible ambiguity attached to the crystallographical characters which we have been desirous of pointing out in our summary.

the case in astronomy. At the time of Ptolemy, the differences of observation, which did not amount to more than three or four minutes, were neglected, and the results thus obtained were false. Tycho directed his attention to the investigation of those phænomena which were within these limits of determination, and he detected clearly-defined inequalities. Bradley narrowed these limits to seconds of a degree, and within these seconds of variation he found two of the most important phænomena which have been discovered by astronomers,—the nutation of the earth's axis and the aberration of light. In the same way, though the chemists of the present day have become able to recognise, to control, and to characterize comparatively the results of the actions exercised by different substances taken in sensible masses, the investigation of the specific properties attached to the imperceptible corpuscles of which they are composed is the most urgent requirement of modern chemistry, and one which offers a prospect of the most important discoveries. This course is, for example, the only one by which, in availing themselves of the rotatory powers, chemists will be able to obtain reliable data for assigning symbolical formulæ to the complex products which constitute partial groups, which are now the subject of so many contradictory interpretations.

M. Pasteur has pointed out in his memoir many details of observation to which he was able to give only a passing attention, having scarcely sufficient quantities of the various substances upon which he operated to establish their fundamental distinctions. In this respect his investigations have been completed, for he has given the most positive evidence of the dissimilarity of the molecular constitution of the isomeric products. But now that this fact is well established, we avail ourselves of it for the purpose of resuming the question under an inverse point of view; that is to say, in reference to those details themselves, which, in the position it has now assumed, appear to us to have a special importance. In fact, the two series of isomeric bodies which he has obtained present these peculiarities,—that the corresponding terms are formed by similar operations under parallel physical conditions, and that they exercise reactions, the results of which, infinitely varied, can be always clearly distinguished. In this similarity of formation and of physical conditions, the molecular dissimilarity which he has established between the terms compared cannot be accounted for except by conceiving that it results from one or more of the three following conditions,—the mass of the chemical corpuscles, their configuration, or the interior arrangement of the similar ingredients of which they are constituted. The supposition of the inequality of the masses is excluded here by the same test which renders it admissible in the case of

the four isomeric products of cyanogen; for the capacities of saturation of the comparative terms are always found to be exactly equal, while in the products of cyanogen they are unequal, and graduate among each other in simple relations, in which the masses of the constituent corpuscles participate. There remain, therefore, only the two other causes of dissimilarity which can be assumed; the diversity of configuration, and that of arrangement, either individually or conjointly. Now here is presented an extremely favourable opportunity of studying their effects with regard to the former, the possibility of tracing it through an infinite variety of combinations, all of which are definite, and nearly all crystallizable; and if this examination should not ultimately lead to their distinction one from the other, it would always be possible to distinguish the characters which appertain to them, together or separately. Such are the important advantages which have not hitherto been offered by any other chemical problem; and we expect of M. Pasteur that he will not fail to follow it up throughout the entire extent of its consequences.

Another investigation, far less difficult, may now become very rich in results for him and all those who may be desirous of entering upon this study. The number of vegetable acids in which the power of rotation has hitherto been observed is very limited. They are the dextrotartaric and levotartaric, their derivative tartarovinic and tartaromethylic acids; the camphoric, camphoramic, aspartic, malic and kinic. It is extremely probable that others will be found possessing, like these, the same power, if they are sought for in the juices of plants collected at different periods of their growth, and in juices of fruits expressed at different stages of their maturation. These would have but little interest if it were only possible to expect isolated results, which could only add one more to this class of bodies so remarkable for their instability. But the presence of the rotatory power in those acids which possess it gives them an entirely different value; for by tracing the modifications of this power in their salts, their æthers, their chemical derivatives, and all the combinations whatever into which they can be made to enter, chemistry, crystallography and molecular optics would thus acquire an abundance of new facts, by which each science would be enriched and its range of view extended. The experimental sciences would be improved by a more true appreciation of already known results, and by the discovery of new facts which would extend their field of application. We point out immediately the first kind of services in the perseverance of M. Pasteur. We now recommend the second to his zeal, and to that of all experimentalists who are capable of joining with him in this study; for there is a sufficient occupation for every one.

After having so long taken up the attention of the Academy with the question of isomerism, which was the principal object of the memoir of M. Pasteur, we must give a more brief summary of the second part, in which he has presented in a few pages some comparative remarks upon the molecular constitution of the malic and tartaric acids. It is not because he did not find very great analogies and very curious phænomena that the author has had reason to mention them. Thus he has proved that the malic acid is, like tartaric, very considerably affected by the nature and proportions of the solvents in which it is observed; that this takes place instantaneously with boracic acid, in all cases with singular differences in the mode and even in the direction of the dispersions. But in the first place, in order to render appreciable the consequences which the results of these optical examinations may suggest or legitimize, it would be necessary to preface them by other remarks which relate to the peculiarities of the action which tartaric acid exercises upon polarized light, and that would extend our report beyond convenient limits. Again, we have a still stronger motive, which is, that the small quantities of malic acid which M. Pasteur had at his disposal did not admit of his following up the optical study with all the detail which he would have desired; and the same difficulty has equally restricted our capability of confirming experimentally the general results which he has announced. But we learn, that, by the kindness of M. Liebig, he is now provided with an abundance of this acid; so that he will be able to resume the complete study with that zeal which distinguishes him; and we shall await the time of its completion before specially entertaining the Academy with an account of what he has already been able to discover with regard to this subject.

XLV. *On the Meteor which appeared on Thursday, the 12th of August* 1852, *at about* 9^h 20^m P.M. *Greenwich Time. By* JAMES GLAISHER, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE brilliant meteor described in your Journal of last month, as seen at Sidmouth, was remarkable in many respects. I beg to send you the following particulars relating to it, with the results of some calculations I have made to determine its distance from the earth.

I. From Belfast. L. P. Duke, Esq., kindly favoured me with the following particulars :—

" I beg to state that the meteor seen on the evening of the 12th instant made its appearance here one or two minutes after 9 o'clock P.M. (Belfast time). Its place when first seen was almost the position of β Capricorni, being in a direct line with α Lyræ and α Aquilæ, and at about the same distance from the latter as between these stars. It continued visible about 10^s, leaving a brilliant train. Its elevation, I should think, was about 12° or 15° above the horizon, and the direction of its flight was N.W."

II. From Belfast. In a second communication L. P. Duke, Esq., says,—

" In reply to your further inquiry respecting the meteor of the 12th of August, I have to say, that, as it appeared to take a course nearly parallel to the horizon (as far as I remember), its altitude at the time of disappearance was about the same as at that of the appearance, which was from 15° to 20°. As it appeared a few degrees only east of south, and took a westerly course, it must have been at the time of disappearance some degrees west of the meridian ; but I regret that, not having accurately marked the place of its appearance, I am unable to speak definitely regarding it."

At Belfast, at this time β Capricorni was situated at an altitude of about 17½°, and in azimuth about 21° east of south ; therefore the most probable altitude of the meteor at Belfast was about 15°.

III. From Dorrington, Glassan Ballymahon, Ireland. St. George Grey, Esq., in a letter to me, says,—

" On the evening of the 12th inst., about 9^h 20^m Greenwich time, I observed in the eastern quarter of the heavens a large meteor, of a vivid blue colour, descending in a southerly direction towards the horizon at an angle of about 45 or 60 degrees. In its colour and time of appearance it corresponds exactly with the meteor mentioned in a late Number of the *Illustrated London News*, and there seems to be no doubt of its being identical with the one described in that journal.

" I did not observe it until a little before its disappearance, but a friend who was along with me describes it as a round body with two trains of light extending from it some degrees in opposite directions. I was fortunately able to mark pretty accurately the place of its disappearance, the altitude of which I measured with a sextant and found to be 9°, and its azimuth eastwards from the south point 72°. My latitude and longitude are 53° 27′ 50″ N., and 7° 45′ W. respectively.

" Though I have not been so successful in obtaining as perfect an observation of this meteor as of several others which I have

seen, I should feel gratified if this communication should verify any conclusion respecting its distance, &c. which you may deduce from the observations of others*."

IV. From Holyhead.   From the *Illustrated London News* of August 21, 1852, by George F. Lyster, Esq.

"I beg to call your attention to an extraordinary meteoric appearance in the heavens, which occurred in this neighbourhood on the evening of the 12th instant.   At 9ʰ 20ᵐ P.M. (Greenwich time) my attention was suddenly attracted by what appeared to me to be a peculiarly vivid flash of lightning; and on turning towards the S.S.W., the direction whence it came, I saw a magnificent body of meteoric light, the colour being a most beautiful and intense blue.

"It occupied at least a fourth part of the visible heavens, inclining towards the earth at an angle of 45°, and lasted for 30 or 40 seconds, gradually diminishing in width to a narrow streak of light, the length remaining the same as when I first saw it. Just previous to its fading away, it became flickering and wavy. The sky at the time was clear and cloudless, and a fresh breeze blowing from the N.N.W."

The bearing S.S.W. in this account is evidently erroneous, as the meteor was situated nearly S.E. of Holyhead.

V. From Pembroke.   Samuel A. Good, Esq., favoured me with the following information:—

"On the 12th of August, at precisely 9ʰ 20ᵐ, I was walking along a street, when a bright flash, as of lightning, attracted my notice.   The houses on my left-hand (east) prevented me from seeing the meteor itself; but when I came to an opening after a few steps, I observed just above θ Andromedæ a wavy bright cloud of a phosphoric appearance.   It gradually faded away, and at the same time appeared as if carried on by the wind at the rate of the other clouds.   It finally disappeared after passing under the star I have mentioned.   There were a good many shooting stars the same evening, which at intervals was very cloudy.

"Two or three persons who saw it at first inform me that it appeared like a large ball, which burst and extended itself in the form described above."

* Whilst this account was passing through the press, the following additional communication was received from Mr. Grey:—"Since I received your letter, I have been trying to discover some one in this neighbourhood who saw the meteor of August 12 at its first appearance, but I am sorry to say without success; however, my impression is, that its altitude and azimuth at that time were respectively in or about 14° or 15° and 81°.   Using these angles of altitude, the former gives a distance of 71 and the latter of 75 miles."

From the diagram accompanying this letter, the meteor when first seen was at the altitude of 33°, and when last seen at an altitude of 30°, and in azimuth $22\frac{1}{2}$° E. of N. at both times.

VI. From Swansea.   Matthew Moggeridge, Esq., favoured me with the following particulars :—

"On August 12, at $9^h\ 22^m$ P.M., sky clear, wind W., while walking in the garden at the Willows, our attention was caught by a sudden glare, and turning round I saw a meteor approaching Cassiopeia from the magnetic north; colour, phosphoric blue, width 15 minutes; length (nucleus) 1 deg. 3 min., followed by a fiery streak 5 deg.   Motion at first that of a rocket; duration 3 minutes 40 seconds, of which perhaps one second was occupied by its transit across 15 degrees of the heavens, from the point where it appeared over the trees at the east end of the house, until it became stationary in a line between my eye and Cassiopeia, the nucleus then being a little above α Cassiopeia. It now assumed a totally different aspect; colour red, changing to that of a luminous vapour; width 30 min., length 8 deg., motion for 2 sec., contorting and coiling itself up until it appeared like a huge snake reposing its massive folds in Cassiopeia's chain. There it remained for the rest of the time that it was visible (*i. e.* 3 min. 37 sec.), always within that constellation, though slightly varying in position from the contortions which continued, but were less marked, until the whole gradually faded away."

Therefore the altitude of the meteor as seen from Swansea was about 40°, and its azimuth about 45° E. of N.

VII. From Torrington, Devonshire.   Henry D. Lowe, Esq., furnished me with the following particulars :—

"I have observed many meteors, but I never witnessed so great an amount of light from a similar cause as surrounded me at the moment that the meteor of the 12th instant made its appearance.   At that time I was about a mile distant from this town in an easterly direction, and on the part of the road where I was walking surrounding objects had become more indistinct than they would have been from the time of night alone, in consequence of an overhanging wood, when in a moment every object was covered with light; but it gave me no impression of its being a flash of lightning; for while the continuance of the light upon the scene around was of short duration, yet it was sufficiently long to contrast it with the lightning's instantaneous flash.

"I was dazzled, and did not at once recognise the object itself; and when I did, it was at a considerable distance, and presented the appearance depicted in the *Illustrated News*.   I have been anxious to meet with some one who saw the body itself at the moment of its appearance, bu I have not been able to do so.

" From the time during which I stood looking at it, and from the distance which I walked while it was still in view, it must have continued in sight for five minutes.

" I particularly noticed its position at the time it disappeared, which was exactly in a line with the nebulæ situated rather more than half-way between α Persei and δ Cassiopeia—so exactly, indeed, that to my view the light of the meteor appeared to blend with, and to be lost sight of in, the light of the nebulæ."

Therefore its altitude was about 31°, and its azimuth about 35° E. of N.

VIII. From Heavitree. John Chintiphers, Esq., favoured me with the following :—

" I beg to inform you that on the 12th inst., at about 9ʰ 15ᵐ P.M. (Greenwich time), between Chudleigh and Exeter, while ascending Haldon Hill from the South, I saw in about a N.N.W. direction, and at about 55° of altitude from the earth, a brilliant meteor of a bluish-yellow colour.

" Its first form was spherical, about one-fourth the size of a full moon, and its course was horizontal from east to west, leaving behind it a stream of light. I chanced at that moment to be looking out of the carriage-window in the direction where it appeared, and I noticed that the sphere continued in sight for perhaps three or four seconds of time, when it suddenly vanished, but the stream of light remained very bright for two or three minutes, and then gradually diminished; so that in about five minutes it had, to the naked eye, wholly disappeared.

" On the sphere vanishing, the stream was probably about 6 or 8 degrees in length, and half a degree in breadth, pointed at both ends.

" The motion of the sphere was not only much less rapid than lightning, but rather less than falling stars. The sky was clear at the time, the wind westerly; and though I omitted to notice its relative position in the heavens, the sphere when first seen was probably north-easterly of Ursa Major.

" The brilliancy of the meteor was so great that it created quite a thrilling sensation in me, and the more so possibly, because that was the third occasion within about half-a-dozen years that I have observed a meteoric appearance while ascending that same hill."

From all accounts the meteor was situated east of the meridian at Heavitree, and the estimated altitude 55° is much too great.

IX. From Devonport. S. Gower, Esq., favoured me with the following :—

" For the use that the testimony of a casual observer may be to you, I write to say that I was standing on Mount Wise, Devonport, on the evening of the 12th instant, and distinctly saw

the meteoric appearance described in the *Illustrated London News*. As I know but little about the stars, I can only add that the extraordinary and beautiful light burst forth within a short distance of the Great Bear."

X. From Charlton Somerton, Somersetshire.   J. R. Page, Esq., favoured me with the following :—

" I beg to inform you that on the evening of the 12th August, about 9ʰ 20ᵐ, at Sidmouth in Devonshire, my attention was suddenly attracted to a very bright light of a splendid blue colour, something in the shape of a fish with a long tail, head part to the north, and the tail inclining downwards.   It gradually faded away, leaving for some time after its disappearance a whitish-looking cloud ; it was visible some thirty or forty minutes. The meteor disappeared in much the same place that it appeared."

XI. Sidmouth. From the Philosophical Magazine for September 1852, by N. S. Heincken, Esq.

" A rough trial with a common quadrant the following day gave, from the position indicated by a sailor who saw the meteor (probably at its greatest altitude), about 30°, and about 10° E. of magnetic north.   It is described by all as having cast a strong shadow—that the hour could have been seen on a watch—that no noise was heard—that while globular it approached the full moon in apparent size."

The western declination of the magnetic needle at Sidmouth is about 23° ; this would place the meteor 13° west of the meridian of Sidmouth, which is discordant with the other observers at Sidmouth : probably it should be 10° west of the magnetic meridian, which would give 33° E. of N. as the azimuth, and which is near the truth.

XII. Sidmouth.   From the Philosophical Magazine for the month of September 1852, by Anne R. Bennett.

" I have written down, according to your request, all the particulars I can remember respecting the beautiful meteor which I saw last evening.   It was about 12 minutes past 9 P.M. when, as I was walking home, a light—so bright that my figure cast a strong shadow across it—streamed upon the road.   I instantly turned round and saw in the sky, about 15° W. of Cassiopeia, a meteor the size of the full moon, and of a warm yellow colour. The ball immediately shot out into a bar, apparently 7° 30′ in length and 1° wide.   The edges of the bar were sharply defined, and the breadth was the same throughout ; but both the ends were jagged, and in the centre there was a rent which gradually became wider, until within 30 seconds, as near as I could guess the time, there was a distance of 30′ between the two proportions.   In about 30 seconds more the bar lost its sharply defined appearance and faded into a thin luminous cloud with a pale

diffused light, which disappeared altogether in the course of two minutes. The meteor appeared to me to be stationary after it had shot out into the bar, until it had faded into the luminous cloud, which I fancied had a slow motion towards the east."

From this account the azimuth of the meteor was about 30° E. of N., and altitude about 30°.

XIII. Sidmouth. From the Philosophical Magazine for the month of September 1852, by H. N. Smith.

"As I was returning home about 12 minutes past 9 P.M. on Thursday the 12th instant, I was startled by seeing the road suddenly illuminated by some brilliant light behind me. Turning instantly I saw a bright body in the sky, due north, I think, half-way between the pole star and the horizon, the constellation Cassiopeia being to the east of it. The meteor rapidly spread into a horizontal bar about $7\frac{1}{2}$ degrees long and scarcely one in width, the centre and extremities of a beautiful pale green—two points between bright yellow shading into a green on either side; the centre became paler, as though a separation were taking place, and gradually the meteor lost its brilliancy and defined form, becoming more like a small luminous cloud which slowly faded from my sight. The meteor appeared stationary, and was visible for 2 minutes, but the intense light lasted scarcely 30 seconds."

XIV. From Stoke Sub Hamden near Yeovil, Somersetshire. From the *Illustrated London News* of August 21, 1852, by Richard Walter, Esq.

"A meteor of an extraordinary appearance was seen here last night (August 12) at about $9^h 20^m$ Greenwich time, to which my attention was directed by a very brilliant flash, as of lightning; and immediately turning my eyes to the north, the body of the meteor (if any) had disappeared, having left a brilliant mass of light about 15 degrees in length. This continued visible for nearly ten minutes, gradually fading; but (what I consider most remarkable) it resolved into two or more distinct bodies nearly of the apparent magnitude of the planet Saturn, which remained so long visible that I almost doubted if there were not stars in that precise spot which I had not noticed. They, however, became less and less visible, and had disappeared in about ten minutes after. I hope to find that this meteor has been noticed by other observers in distant parts of the world, and its place correctly marked, which may lead to a calculation of its distance, &c."

XV. From Stoke Sub Hamden near Yeovil, Somersetshire. Richard Walter, Esq., favoured me with the following :—

"I beg to inform you that the position of the meteor of August 12, pointed out in the diagram of R. W., is strictly cor-

rect, but I think forming too small an angle with the horizon—probably it was about 30°—certainly not so much as stated by George Fosbery Lyster, Esq. The centre of the object was as nearly as possible 34° in altitude, and, as may be seen by the diagram, nearly N. by W. I have not heard of any one having seen the body which produced the illumination; and it appears to me unaccountable that the appearance should continue so long visible, and its resolution into distinct luminous masses, which has been described to me by other observers agreeing with my own perception; and these had not wholly disappeared until nearly ten minutes had elapsed.

" The observation of Mr. Lyster could not, I think, be correct as to the position of S.S.W., as my attention was directed to it immediately after the flash of lightning, and it was then, as above mentioned, N. by W."

XVI. Third communication from Mr. Walter.

" On receiving your favour I have endeavoured to set down as correctly as possible the position of the meteor of August 12, and I find that my former statement was incorrect from my hastily laying down the position of the stars as to time, which in the enclosed diagram I have done with more accuracy. The azimuth of the meteor was certainly not W. of N., but a little E. of N. Its position was marked by me with reference to Polaris and α Ursæ Majoris, with which its centre formed an angle a little more than 90°; and as I had ample time to mark it repeatedly, I believe the position shown in the diagram to be strictly correct."

XVII. From Bristol. Thomas Austin, Esq., furnished me with the following particulars :—

" About 9ʰ 30ᵐ P.M. (local time), on the night of the 12th inst., a vivid flash of lightning, or a similar evanescent burst of light, was observed to issue from a dense black cloud in the north. This black cloud was fringed with offsets of less density; out of one of these, or rather through it, after the lightning flash had passed away, an horizontal streak of intensely bright light was projected, which continued to display its luminosity for about ten minutes, when it gradually faded away; before finally disappearing it resolved into three portions.

" The streak of meteoric light was nearly north, and at an elevation of about 65° above the horizon."

The estimated altitude is too great.

XVIII. Second communication from T. Austin, Esq.

" On receiving your communication I revisited the spot from which the meteor of the 12th ultimo had been seen by me, but regret I cannot give you the altitude, as the objects around by

which I could estimate its exact position were not sufficiently noticed at the time to enable me to do so now with any degree of accuracy; and when 'about north' was mentioned as the position of the meteor at the time of its disappearance, it was merely conjecture as to true bearing, as I had no instrument by which its correct position could be ascertained. One of my sons, who was about a mile and a half on the south side of Bristol at the time the meteor appeared, assures me that its bearing was certainly to the east of north; and as he is an engineer, and has surveyed in the locality from which he observed, and is therefore familiar with the bearing of different objects around, I have no doubt that it was to the eastward of north.

"The light from the meteor was so great that the smallest objects could be discerned as clearly as in full daylight."

XIX. From Christchurch. A. P. Falconer, Esq., furnished me with the following particulars :—

"On the 12th of August, at $9^h\ 20^m$ P.M., I was on my way home, and meeting a person whom I stopped to speak to, we were suddenly surprised by an intense mild light which illuminated everything around as clear as day; on turning round to see from whence it proceeded, I saw a body rapidly passing from east to west, leaving a long train of light and sparks in its path, and disappearing. The duration of the light was hardly 3 or 4 seconds, but the light emitted in its train and passage might have been 10 seconds. The angle of its appearance was 45° or less; it passed below the polar star from the eastward.

"The duration of this meteor was so brief, that a person had hardly time to turn and view it for so long a period as many assert, though an intervening cloud or haze might have hid this interesting phænomenon from me."

XX. From Christchurch. Elias Lane, Esq., favoured me with the following information :—

"I happened to be in an open field on the evening of the 12th of August, and was attracted by the flash described by other observers to the part of the heavens from whence it proceeded; and facing, as I was, the north, I had a full view of it, similar in every respect to the observation of G. F. Lyster, Esq., only I think it must have been to my observation much smaller; it was, as near as I could notice, about half a degree west of, and one degree below, the polar star; it lasted about a minute, and then died away slowly and gradually.

"I do not know that it was seen by any other person in this town or neighbourhood; many persons noticed the flash, but thinking it to be that of lightning took no further notice of it."

Second communication from E. Lane, Esq.

"I beg to say that the meteor appeared to me to have no motion whatever, but seemed like the sudden opening and gradual closing of the heavens."

From both these accounts it would seem that the meteor was situated very nearly on the meridian of Christchurch, but it seems to have passed west of the meridian from all other accounts. In both accounts the meteor is referred to as occupying a position very near to Polaris, giving an altitude of nearly 49°, which is certainly much too great.

These are all the accounts I have received giving information upon this meteoric appearance, and I now proceed to discuss them, for the purpose of determining its distance from the earth, &c.

1. *Determination of the Spot over which the Meteor was vertical at its disappearance.*

From the greater number of the accounts, it seems that the meteor was almost stationary for a very considerable time before it disappeared, and it would seem that it was occupying this position when its place was noted by most of the observers.

The observations available for this determination are those at Dorrington in Ireland; at Pembroke, Swansea, Torrington, Sidmouth, Yeovil, Bristol and Christchurch.

At Dorrington its azimuth was		72° E. of S.
At Pembroke	...	22½ E. of N.
At Swansea	...	45 E. of N.
At Torrington	...	35 E. of N.
At Sidmouth	...	30 E. of N.
At Yeovil	...	little E. of N.
At Bristol	...	E. of N.
At Christchurch	...	nearly N.

The intersection of these lines, excepting that at Christchurch, is at a spot very near to Worcester; and from the close agreement of so many observers differently situated, the meteor at the time of its disappearance must have been nearly in this position.

Determination of the Distance of the Meteor from the Earth for some time before and at its disappearance.

The data we have for this determination are the following:—

At Belfast, at the distance of 224 miles, its altitude was				15°
At Dorrington	...	247	...	9
At Pembroke	...	112	...	30
At Swansea	...	79	...	40
At Torrington	...	98	...	31
At Sidmouth	...	112	...	30
At Yeovil	...	86	...	34

Hence, taking into account the curvature of the earth, from the observation at—

Belfast its height was		67	miles.
Dorrington	...	47	...
Pembroke	...	67	...
Swansea	...	67	...
Torrington	...	62	...
Sidmouth	...	67	...
Yeovil	...	59	...

As the altitudes in all cases were either measured, or determined by reference to adjacent stars, each of these results are entitled to equal weight; but it is evident that the angle at Dorrington was too small, although measured by a sextant. If this result be omitted, the resulting distance from the earth is 65 miles; if included, it is 62 miles. The former result is probably the more accurate.

The observations do not furnish data for determining the path of the meteor in the heavens, except that it was moving towards the west, and approaching the earth; when first seen by the observer at Pembroke it was at an altitude of 33°, and therefore was about 75 miles from the earth. The observer who first saw the meteor was Matthew Moggeridge, Esq., at Swansea, and this gentleman agrees with others in the almost fixed position of the meteor for some time before its extinction. There is a remarkable agreement as to the colour of the meteor; all speak of it as being of an intense blue; from the observers at the most distant stations, who must have viewed it under very different circumstances respecting the clearness of the atmosphere, to those who saw it at a high elevation.

The brilliancy of the meteor is spoken of as extreme.

The size of the meteor seems to have been very great, both from the general accounts and from sketches which have been furnished to me.

At the time of this meteor's appearance the sky all over England, except in the west and south, was covered by dark cloud; I have therefore failed to obtain any accounts, except at places situated on the west and south coasts.

XLVI. *On Continued Fractions in Quaternions. By* Sir WILLIAM
ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S.&c., Andrews'*
Professor of Astronomy in the University of Dublin, and Royal
Astronomer of Ireland*.

[Continued from vol. iii. p. 373.]

3. **I**T results from what has been shown in the two former
articles of this paper, that, whether in quaternions† or
in ordinary algebra, the value of the continued fraction,

$$u_x = \left(\frac{b}{a+}\right)^x c, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

may be found from the equation

$$\frac{u_x - u''}{u_x - u'} = v_x, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

where

$$v_x = u''^x \frac{c - u''}{c - u'} u'^{-x}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

or from the expression

$$u_x = (1 - v_x)^{-1}(u'' - v_x u'), \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

if u' and u'' be two unequal roots of the quadratic,

$$u^2 + ua = b. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

If, then, $a\ b\ c\ u'\ u''$ be five real quaternions, of which the three
last are unequal among themselves, and the two latter have
unequal tensors,

$$\mathrm{T}u' > \mathrm{T}u'', \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

we shall have the following limiting values:

$$\mathrm{T}v_\infty = 0, \quad v_\infty = 0, \quad u_\infty = u''. \quad \cdot \quad \cdot \quad (7)$$

We may then enunciate this Theorem :—If the real quaternion c
be *not a root* of the quadratic equation (5) in u, *the value of the
continued fraction* (1) *will converge indefinitely towards that one
of the real quaternion roots of that quadratic, which has the lesser
tensor.* If the quaternion c or u_0 be a root of that equation, it
is clear that the fraction will be constant.

September 21, 1852.

[To be continued.]

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* Communicated by the Author.
† The writer has again to regret that unforeseen causes of delay have
occurred to retard the publication of his Volume of Lectures on Quater-
nions, of which, however, he hopes that the value will be found to have
been increased, by the additions which he has inserted.

XLVII. *On a Universal Tendency in Nature to the Dissipation of Mechanical Energy.* By Prof. W. Thomson *.

THE object of the present communication is to call attention to the remarkable consequences which follow from Carnot's proposition, that there is an absolute waste of mechanical energy available to man when heat is allowed to pass from one body to another at a lower temperature, by any means not fulfilling his criterion of a " perfect thermo-dynamic engine," established, on a new foundation, in the dynamical theory of heat. As it is most certain that Creative Power alone can either call into existence or annihilate mechanical energy, the "waste" referred to cannot be annihilation, but must be some transformation of energy†. To explain the nature of this transformation, it is convenient, in the first place, to divide *stores* of mechanical energy into two classes—*statical* and *dynamical*. A quantity of weights at a height, ready to descend and do work when wanted, an electrified body, a quantity of fuel, contain stores of mechanical energy of the statical kind. Masses of matter in motion, a volume of space through which undulations of light or radiant heat are passing, a body having thermal motions among its particles (that is, not infinitely cold), contain stores of mechanical energy of the dynamical kind.

The following propositions are laid down regarding the *dissipation* of mechanical energy from a given store, and the *restoration* of it to its primitive condition. They are necessary consequences of the axiom, " *It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.*" (Dynam. Th. of Heat, § 12.)

I. When heat is created by a reversible process (so that the mechanical energy thus spent may be *restored* to its primitive condition), there is also a transference from a cold body to a hot body of a quantity of heat bearing to the quantity created a definite proportion depending on the temperatures of the two bodies.

II. When heat is created by any unreversible process (such as friction), there is a *dissipation* of mechanical energy, and a full *restoration* of it to its primitive condition is impossible.

III. When heat is diffused by *conduction*, there is a *dissipation* of mechanical energy, and perfect *restoration* is impossible.

IV. When radiant heat or light is absorbed, otherwise than in

* From the Proceedings of the Royal Society of Edinburgh for April 19, 1852. Communicated by the Author.

† See the Author's previous paper on the Dynamical Theory of Heat, § 22.

vegetation, or in chemical action, there is a *dissipation* of mechanical energy, and perfect *restoration* is impossible.

In connexion with the second proposition, the question, *How far is the loss of power experienced by steam in rushing through narrow steam-pipes compensated, as regards the œconomy of the engine, by the heat* (containing an exact equivalent of mechanical energy) *created by the friction?* is considered, and the following conclusion is arrived at:—

Let S denote the temperature of the steam (which is nearly the same in the boiler and steam-pipe, and in the cylinder till the expansion within it commences); T the temperature of the condenser; μ the value of Carnot's function for any temperature t; and R the value of

$$\epsilon^{-\frac{1}{J} \int_T^S \mu dt}.$$

Then $(1-R)w$ expresses the greatest amount of mechanical effect that can be œconomized in the circumstances from a quantity $\frac{1}{J}w$ of heat produced by the expenditure of a quantity w of work in friction, whether of the steam in the pipes and entrance ports, or of any solids or fluids in motion in any part of the engine; and the remainder, Rw, is absolutely and irrecoverably wasted, unless some use is made of the heat discharged from the condenser. The value of $1-R$ has been shown to be not more than about $\frac{1}{4}$ for the best steam-engines, and we may infer that in them at least three-fourths of the work spent in any kind of friction is utterly wasted.

In connexion with the third proposition, the quantity of work that could be got by equalizing the temperature of all parts of a solid body possessing initially a given non-uniform distribution of heat, if this could be done by means of perfect thermo-dynamic engines without any conduction of heat, is investigated. If t be the initial temperature (estimated according to any arbitrary system) at any point xyz of the solid, T the final uniform temperature, and c the thermal capacity of unity of volume of the solid, the required mechanical effect is of course equal to

$$J \iiint c(t-T)dx\,dy\,dz,$$

being simply the mechanical equivalent of the amount of heat put out of existence. Hence the problem becomes reduced to that of the determination of T. The following solution is obtained:—

$$T = \frac{\iiint \epsilon^{-\frac{1}{J}\int_0^t \mu dt} ct\,dx\,dy\,dz}{\iiint \epsilon^{-\frac{1}{J}\int_0^t \mu dt} c\,dx\,dy\,dz}.$$

If the system of thermometry adopted* be such that $\mu = \dfrac{J}{t+\alpha}$, that is, if we agree to call $\dfrac{J}{\mu} - \alpha$ the *temperature* of a body, for which μ is the *value of Carnot's function* (α and J being constants), the preceding expression becomes

$$T = \frac{\iiint c \, dx \, dy \, dz}{\iiint \dfrac{c}{t+\alpha} dx \, dy \, dz} - \alpha.$$

The following general conclusions are drawn from the propositions stated above, and known facts with reference to the mechanics of animal and vegetable bodies :—

1. There is at present in the material world a universal tendency to the dissipation of mechanical energy.

2. Any *restoration* of mechanical energy, without more than an equivalent of dissipation, is impossible in inanimate material processes, and is probably never effected by means of organized matter, either endowed with vegetable life or subjected to the will of an animated creature.

3. Within a finite period of time past the earth must have been, and within a finite period of time to come the earth must again be, unfit for the habitation of man as at present constituted, unless operations have been, or are to be performed, which are impossible under the laws to which the known operations going on at present in the material world are subject.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 236.]

May 13, 1852. "REPORT of the general process adopted in Graduating and Comparing the Standard Meteorological Instruments for the Kew Observatory." By Mr. John Welsh. Communicated by Col. Sabine on the part of the Committee of Recommendations of the Government Grant.

In offering to the Committee a short statement of the progress made at the Kew Observatory in the construction and verification

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* According to "Mayer's hypothesis," this system coincides with that in which equal differences of temperature are defined as those with which the same mass of air under constant pressure has equal differences of volume, provided J be the mechanical equivalent of the thermal unit, and $\dfrac{1}{\alpha}$ the coefficient of expansion of air.

of thermometers, I shall first describe generally the method pursued in the graduation of standard instruments.

The plan of operations hitherto adopted has been that proposed by M. Regnault, and consists essentially of the following steps :—
1st. Calibration of the tube : 2nd. Graduation of the scale : and 3rd. The determination of the scale coefficients.

1. *Calibration.*—A tube having been selected as being tolerably free from all visible defects, a short column of mercury, generally less than one inch in length, is introduced. The tube is then attached to the frame of Perreaux's dividing-engine, and by means of flexible tubing is put in connection at both ends with india-rubber bags, the pressure upon which can be regulated by means of screws. The mercury is then brought to the part of the tube where the gradua-tion is proposed to commence. The cutting-frame of the engine carries also a small microscope with cross wires in its focus; on turning the dividing-screw, the microscope-wire is brought to coin-cide with the first end of the mercury, and the screw is then turned forward until the wire reaches the second end; the length of the column is thus given in revolutions of the screw. By means of the india-rubber bags, the mercury is made to move along the tube until the first end coincides again with the microscope-wire; the length of the column is again measured, and the mercury again moved for-ward; the same process being repeated until the column has been measured for each length of itself through the whole extent of the proposed scale. Permanent marks are made on the glass at the points of commencement and ending of the calibration. If the pro-gress of the numbers shows any considerable irregularity in the tube, and as a verification of the first set of measures, it is well to repeat the calibration, commencing in this case at a point one-half the length of the column in advance of the original starting-point. A series of measures interpolated from the two sets may then be adopted. Some experience is necessary in order to bring with faci-lity the end of the mercury exactly to the wire of the microscope; but when care is taken to use very pure mercury and clean tubes, the operation can generally, after a little trouble, be accomplished with much accuracy. M. Regnault, I believe, recommends that the motion of the mercury should be regulated by the breath, a drying substance being interposed to prevent moisture entering the tube. This method was employed for some of the first instruments made at Kew, but was abandoned in favour of the elastic bags.

2. *Graduation.*—The measured lengths of the column of mercury in its successive steps along the tube correspond to equal volumes. Assuming that the calibre of the tube does not vary throughout the small length of the calibrating column, if we divide the spaces oc-cupied successively by the mercury into an equal number of parts, it is evident that the divisions will represent the same *capacity*, although they may be of very different *lengths*. Before making the tube into a thermometer, the divisions of the scale may be verified by introducing a longer column of mercury, and examining whether the column occupies an equal number of divisions in different parts

of the scale. If there should be any irregularity, a table of corrections may readily be formed. It will generally be found, however, that if the operations have been performed with care, and the tube is not very faulty, no correction will be necessary. The divisions are cut with a fine needle-point upon a coating of engravers' varnish, and afterwards etched with fluoric acid. The required dimensions of the bulb may be found approximately by weighing a measured length of the mercurial column, and from the known expansion of mercury and its specific gravity computing the capacity of the bulb.

3. *Determination of the scale coefficient.*—The thermometer having been filled with mercury, we have an instrument the divisions of whose scale represent equal increments of the volume of the fluid, but are entirely of an arbitrary value. If now we determine the points of the scale at which the mercury stands in freezing and boiling water, we can immediately convert the arbitrary scale-readings into degrees of the ordinary scales of temperature. If a be the scale-reading for the freezing-point, and b that for the boiling-point, the temperature by Fahrenheit's scale corresponding to any reading $n = \left(\dfrac{n-a}{b-a}\right)180 + 32$. The freezing-point is determined by placing the thermometer in finely-pounded ice, from which the water is drained off as it melts. The boiling-point is ascertained by the form of apparatus employed by M. Regnault; the temperature observed is that of steam, whose elasticity is the same as that of the atmosphere. A small siphon water-gauge communicating with the interior of the vessel gives notice to the observer when the ebullition is being carried on too rapidly. The steam is generated from distilled water. The height of the barometer is observed at the time of the experiment, and the correction to a uniform height of 30 inches (reduced to 32°) is found from Regnault's table. In determining the fixed points, the stems of the thermometers are kept vertical; if the subsequent comparisons with other instruments are made in the same position, no error will arise from the expansion of the bulb caused by the pressure of the column of mercury. If, however, the thermometers are intended to be used in any other than a vertical position, it becomes necessary to determine the fixed points also in a horizontal position.

In accordance with the plan here sketched, fifteen thermometers have been completed with arbitrary scales. About thirty more tubes have been calibrated, and the bulbs attached and filled, but the scales not yet divided. The principal object in graduating the tube with an arbitrary scale is the convenience it affords of testing the divisions before it is converted into a thermometer. It is now proposed to divide the scale at once into Fahrenheit degrees after the thermometer has been made, and to test the accuracy of the divisions afterwards by detaching a portion of the mercurial column and making it move along the tube. If the scale should not then be found correct, a table of its errors can be formed and furnished with the instrument, or the thermometer rejected. The scales of these thirty thermometers have not yet been proceeded with, as it is

desirable, before doing so, to allow the freezing-point to have attained a permanent position. A few divisions have been cut on the tubes near the freezing-point, and the reading with reference to this short arbitrary scale taken from time to time in melting ice. The period elapsed since the construction of the thermometers has been too short to afford as yet much information as to the probable constancy of the freezing-points. They have, however, already shown generally a tendency to rise, in some cases to the extent of nearly $0°·3$ Fahr., but in most of them it does not yet exceed $0°·1$ or $0°·2$. Another peculiarity in connection with the freezing-point has shown itself in almost all the thermometers yet tried. After a thermometer has been exposed for some weeks to the ordinary temperature of the air, if its freezing-point be ascertained, and it be then suddenly exposed for a short time to the temperature of boiling water, and again immediately placed in ice, it is found that the latter determination of the freezing-point will be *lower* than the former by a very appreciable amount, generally between $0°·1$ and $0°·2$ Fahr. The freezing-point does not recover its previous position for some time, probably two or three weeks. This peculiar displacement of the freezing-point has been found to take place also in the case of a standard by Troughton and Simms belonging to the Royal Society. The freezing-point of this instrument, before being raised to the temperature of boiling water, was $32·25$, afterwards it had fallen to $32·15$. This displacement of the freezing-point has been remarked by Mr. Sheepshanks in the course of his experiments on standard thermometers[*]. From the experiments now in progress, it is to be hoped that, after a time, some approximation may be made to the laws of these perplexing phænomena.

The apparatus employed for comparing the indications of different thermometers, consists of a cylindrical glass vase 15 inches deep and $8\frac{1}{2}$ inches in diameter,—a stand for supporting the thermometers under comparison, and a means of agitating the water in such a way as completely to assimilate the temperature throughout the vessel. The stand for the thermometer is a vertical rod, supported by a small tripod resting on the bottom of the vase. The thermometers are suspended from hooks sliding on this rod, and adjustable to any height; they are arranged, with their bulbs at the same height in a circle 3 inches diameter round the rod, and kept fixed with sufficient firmness below by being strapped with elastic bands against a projecting six-rayed frame attached to the supporting rod. Six thermometers of almost any form and length can thus be compared at once. The agitator is a flat ring of tinned iron, about 2 inches broad, fitting easily within the vase, and connected by four light rods with a similar ring at top, which serves as a handle. A packing of india-rubber is placed on the outer rim of the plunger to prevent jarring against the glass. The flat tin ring is cut half across at several places, and the corners bent in various ways, so that when moved upwards and downwards the water is driven in *all* directions. The

[*] This fact, I find, is also mentioned in Faraday's "Chemical Manipulation," edit. 1827, p. 139.

dimensions of the agitator are so arranged, that no part of it can possibly touch the thermometers when in operation. The vase, containing water, the stand with thermometers, and the agitator, is mounted upon a wooden revolving stand. The depth of water in the vase is always sufficient to include the whole of the column of mercury, the scales being observed through the water. In taking the observations, the observer, after agitating the water briskly for some time, turns the revolving stand till each thermometer is brought successively opposite to his eye, reading off the scales as quickly as possible to an assistant, who writes down the numbers. Proceeding in this way, I find that six thermometers can be read off and recorded easily in 20 seconds. It is of course desirable to make more than one set of readings for each temperature; and in order to avoid as much as possible the changes which may occur during the reading off, it is well to reverse the order of observing the instruments, that is, to read them alternately in the order one to six, and six to one.

The following table contains the results of comparisons of six thermometers, and will show the accuracy which may be obtained by the method of comparison just described; it will also exhibit the accordance in the indications of instruments graduated according to Regnault's process. Each result is the mean of six comparisons. No optical assistance was used in reading off the scales. The freezing-points of all the instruments were determined on the same day, after the comparisons were made.

Results of Comparisons of various Thermometers, March 19, 1852.

| Standard Thermometers. | | | | | | | Barrow, E.I.C., S 7, No. 4. | | Newman (Makerstoun). | | Troughton and Simms (Royal Society). | |
| Kew No. 4. | | Kew No. 14. | | Fastré 231 (Regnault). | | Temperature from mean of standards. | | | | | | |
Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.		Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.
°	°	°	°	°	°	32·00	32·05	+0·05	32·05	+0·05	32·25	+0·25
38·69	−0·02	38·73	+0·02	38·72	+0·01	38·71	38·91	+0·20	38·86	+0·15	38·96	+0·25
45·05	+0·01	45·03	−0·01	45·03	−0·01	45·04	45·30	+0·26	45·18	+0·14	45·30	+0·26
49·96	0·00	49·97	+0·01	49·96	0·00	49·96	50·34	+0·38	50·23	+0·27	50·23	+0·27
55·33	−0·02	55·35	0·00	55·37	+0·02	55·35	55·87	+0·52	55·75	+0·40	55·62	+0·27
60·07	+0·01	60·06	0·00	60·05	−0·01	60·06	60·65	+0·59	60·58	+0·52	60·34	+0·28
65·39	−0·01	65·39	−0·01	65·41	+0·01	65·40	65·99	+0·59	66·03	+0·63	65·65	+0·25
69·93	0·00	69·92	−0·01	69·95	+0·02	69·93	70·57	+0·64	70·67	+0·74	70·22	+0·29
74·69	0·00	74·68	−0·01	74·69	0·00	74·69	75·39	+0·70	75·54	+0·85	75·02	+0·33
80·08	+0·02	80·03	−0·03	80·06	0·00	80·06	80·78	+0·72	81·00	+0·94	80·44	+0·38
85·30	−0·01	85·30	−0·01	85·33	+0·02	85·31	86·10	+0·79	86·25	+0·94	85·75	+0·44
90·50	0·00	90·49	−0·01	90·51	+0·01	90·50	91·36	+0·86	91·47	+0·97	90·87	+0·37
95·29	+0·04	95·23	−0·02	95·24	−0·01	95·25	96·15	+0·90	96·32	+1·07	95·72	+0·47
101·78	+0·01	101·76	−0·01	101·77	0·00	101·77	102·71	+0·94	103·04	+1·27	102·26	+0·49
109·21	+0·05	109·11	−0·05	109·15	−0·01	109·16	110·08	+0·92	110·62	+1·46	109·58	+0·42
						212·00					212·47	+0·47

The thermometers "Kew No. 4" and "Kew No. 14," were graduated on the stems by myself with arbitrary scales : the bulb of No. 4 is spherical, and is about $\frac{3}{4}$ inch diameter ; that of No. 14 is cylindrical, $\frac{3}{4}$ inch long and $\frac{1}{4}$ inch diameter, and very sensitive. "Fastré No. 231 (Regnault)" is a standard by Fastré of Paris, also graduated on the stem with an arbitrary scale according to Regnault's process. This instrument was examined and approved by M. Regnault ; the determination by him of the scale coefficient agreed closely with that afterwards made at Kew. The bulb is cylindrical, about $1\frac{1}{2}$ inch long and $\frac{1}{4}$ inch diameter. "Barrow, E.I.C., S 7, No. 4," is one of a number of thermometers made for the East India Company and sent to Kew for examination. Its scale is of brass divided to degrees. "Newman (Makerstoun)" is the instrument which was supplied to the Makerstoun Observatory as a standard, and to whose indications the results of the temperature observations made there since 1841 have been " corrected." It was, at my suggestion, sent to Kew by Sir Thomas Brisbane for comparison with our standards. " Troughton and Simms (Royal Society) " is a standard belonging to the Royal Society. As its scale extends to above 212, its boiling-point was examined in the same apparatus employed for the Kew standards, its brass scale remaining attached to the tube. It was found to read 212°·7 when the barometer, reduced to 32°, stood at 30·136 inches.

The errors of a thermometer which has been already carefully examined between 32° and about 100°, may be obtained with considerable accuracy for temperatures below 32°, without using a freezing mixture, by the following process. Detach from the column of mercury a portion which will occupy about 40 or 50 degrees of the scale : bring this column within the known part of the scale. Let a, b be the readings at the upper and lower ends respectively ; α, β the index errors at these points as determined by comparison with a standard. Move the column until its lower end coincides with some degree below 32°, the upper end being within the compared portion of the scale. Let c, d be the scale-readings for the upper and lower ends in the new position, γ being the scale error corresponding to c. The error of the scale at d will then be

$$d-\{\overline{c-\gamma}-(\overline{a-\alpha}-\overline{b-\beta})\}\cdot$$

The true length of the detached column may be obtained with increased accuracy by taking a mean of several measures within the known part of the scale. This method was adopted for " Newman (Makerstoun) " and " Troughton and Simms (Royal Society)," and the following errors obtained :—

Newman (Makerstoun).		Troughton and Simms (R.S.).	
Temperature.	Error.	Temperature.	Error.
0̊·7	−0̊·05	5̊·1	+0̊·14
6·2	−0·08	10·0	+0·17
10·7	−0·12	15·0	+0·16
14·6	−0·10	20·0	+0·16
20·2	−0·04	24·8	+0·16
25·8	0·00		

The error of Newman had been previously found, by comparing with a standard in a freezing mixture at −3°, to be inappreciable.

Mr. Welsh's Report, No. 2.

" On the Graduation of the Thermometers supplied from the Kew Observatory for the use of the Arctic Searching Expedition under Sir Edward Belcher."

These instruments were twelve in number, seven mercurial and five spirit thermometers, graduated for low temperatures. The processes adopted for the two kinds of instruments being different, I shall describe them separately.

I. *Mercurial Thermometers.*—These were divided into degrees of Fahrenheit's scale in the following manner :—The tube was first calibrated in the way already described in my former report on the construction of thermometers (dated March 22) ; marks being made on the tube at each extremity of the calibrated space. The bulb was then made and the mercury introduced by the glass-blower, the dimensions of the bulb and the quantity of fluid being made as nearly as possible to correspond with the scale proposed to be made. The thermometer was then placed in melting ice and the freezing-point approximately set off with an ink mark ; a similar temperature being also made for a temperature of between 95° and 100°. A short arbitrary scale of four or five divisions was then divided at each of those points. The thermometer was then again placed in ice and the freezing-point determined accurately with reference to the lower short scale ; and comparisons with two standard thermometers in water determined the value of the upper short scale. Let OP be the calibrated portion of the tube, O being the point of commencement, F the

O   F   H   P

freezing-point as determined by means of the short arbitrary scale, and H the higher point of the scale similarly obtained. Let the distances OF, FH, HP be measured by the screw of the dividing engine. Let R_1, R_2, R_3, &c., R_f, &c., R_h, &c. be the lengths, in revolutions of the dividing screw, of the calibrating column of mercury for each successive step in its progress along the tube during the process of calibration ; R_f being the length of the step in which the point F occurs, and R_h that in which H occurs. The values of R_1, R_2, &c. have been registered in the process of calibration ; OF and OH have been obtained independently ; the second measurement of OP, when compared with the sum of all the R's, will show with what exactness* the column of mercury has been passed through its own length in its progress along the tube. Let r_f be the number

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* If this should be found slightly in error, it can produce no appreciable error in the graduation, as an error could only arise from the alteration of the tube's capacity, which might take place in a length equal to the difference found, this difference being in perhaps no case more than $\frac{1}{100}$ or $\frac{1}{50}$ inch.

of revolutions between the first end of the step f and the point F, and similarly r_h for the step h. We have then

$$OF = R_1 + R_2 + R_3 + \&c. + R_{f-1} + r_f$$
$$\text{and } OH = R_1 + R_2 + \ldots \ldots + R_{h-1} + r_h;$$

whence we obtain r and r_h. Let K be the number of degrees equivalent to one length of the calibrating column,—this being of course constant for each length along the tube on the supposition of equal increments of volume for equal increments of temperature. Also, if we suppose that the capacity of the tube does not vary throughout the length of a single calibrating step, $\dfrac{r_f}{R_f}$ and $\dfrac{r_h}{R}$ will give the fractional parts of a step by which the points F and H are respectively in advance of the first ends of the steps f and h. We have then

$$OF = \left(\overline{f-1} + \frac{r_f}{R_f}\right)K, \quad OH = \left(\overline{h-1} + \frac{r_h}{R_h}\right)K;$$

and $FH = \left(h - f + \dfrac{r_h}{R_h} - \dfrac{r_f}{R_f}\right)K = T_h$, the higher temperature $-32°$;

whence $K = \dfrac{T_h}{h - f + \dfrac{r_h}{R_h} - \dfrac{r_f}{R_f}}$. The degree corresponding to the point

O is $32° - \left(\overline{f-1} + \dfrac{r_f}{R_f}\right)K$. The length of one degree for any individual step x is $\dfrac{R_x}{K}$.

From the quantities thus obtained, a table may readily be formed showing the value in revolutions of the dividing screw of one degree at all parts of the scale, and the graduation may then be proceeded with accordingly. The graduation is carried from $-40°$ to $+120°$ or $130°$ Fahr.

II. *Spirit Thermometers.*—In the graduation of mercurial thermometers, the practice is to consider the increments of volume to be proportional to increments of temperature. If this were assumed in the case of spirit thermometers, very serious errors would be the result, even within moderate ranges of temperature. Dr. Miller having considered alcohol, as on the whole, the best fluid for thermometers intended to measure very low temperatures, I was supplied by him with some which he had himself prepared with great care, its specific gravity being 0.796 at $60°$ Fahr. The first step to be taken was the determination of the *law* of expansion of the fluid in glass, as compared with that of mercury. For this purpose a tube was calibrated and divided with an arbitrary scale according to Regnault's process: its divisions were found, upon verification, to be of exactly equal capacity throughout. The tube was then furnished

with a bulb of the same dimensions as those intended to be supplied to the Admiralty, and filled with the alcohol. This thermometer was marked S. 9 E. Comparisons were then made between the readings of this instrument and those of a standard mercurial thermometer, through as large a range of temperature as was found practicable. The comparisons above the freezing-point were taken in water, in the apparatus described in my former report; those below 32° were taken in freezing mixtures of ice and salt or chloride of calcium. The following Table contains the results of two series of experiments; the numbers in the first two columns are differences from the freezing-point; those in the first being Fahrenheit's degrees; and in the second and third columns, the arbitrary scale divisions of the spirit thermometer S. 9 E.

Table, containing results of comparisons between a Standard Mercurial Thermometer, and a Spirit Thermometer with an arbitrary scale of uniform capacity.

First Series.			Second Series.		
Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.	Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.
°	Scale div.	Scale div.	°	Scale div.	Scale div.
+69·95	+209·5	+0·2	+65·76	+196·4	+0·2
+66·93	+199·7	−0·1	+60·04	+178·3	0·0
+53·15	+156·7	−0·3	+52·04	+153·5	−0·1
+40·53	+118·2	−0·3	+37·72	+110·3	+0·2
+20·83	+ 60·1	+0·2	+24·05	+ 69·8	+0·4
+17·80	+ 51·0	−0·1	+16·01	+ 46·7	+0·8
−18·44	− 50·5	+0·9	−16·38	− 44·8	+0·9
−36·15	− 98·0	+1·2	−29·00	− 79·7	+0·4
−43·14	−117·9	−0·2	−36·33	−100·2	−0·5
			−44·72	−123·0	−1·1

To deduce the law of expansion from these comparisons, the numbers were arranged in equations of the form

$$AT + BT^2 - N = 0, \quad \dots \dots \dots \dots \dots \dots \quad (1.)$$

where T is the number of Fahrenheit's degrees from 32°, N the corresponding number of divisions by thermometer S. 9 E., A and B being the constants whose value is to be ascertained: the constants depending on higher powers of T than the second, were not considered.

The values of A and B were obtained from the equations by the method of least squares, and were as follows:—

From first series .... A = 2·8203 ...... B = 0·002455
From second series .... A = 2·8377 ...... B = 0·002221
The mean of both series giving .. A = 2·829 ...... B = 0·002338

The numbers in the columns "Observed *minus* calculated," are obtained by taking the difference between the observed readings of the spirit thermometer, and the numbers calculated from the *mean* values of A and B just stated.

Having determined upon the adoption of the law of expansion

stated above, the graduation of the spirit thermometer was proceeded with as follows.

The process of calibrating the tubes was the same as for the mercurial thermometers; as in these, also, the freezing-point and a temperature of 90° or 95° were determined with reference to short scales on the stems; the distances OF, OH (figure, page 312) were also measured; and by comparing these measurements with the numbers obtained by calibration, they were expressed in terms of lengths of the calibrating column.

The equation (1.) may be put under the form $N = A(T + \theta T^2)$ by making $\theta = \dfrac{B}{A}$. Let f and h be the distances OF, OH expressed in steps of the calibrating column; $FH = h - f$. Let T_h be the number of degrees above 32° corresponding to H, and let α_0 be the value, in terms of a calibrating step, of one degree at the temperature 32°: we have then, according to the fundamental equation (1.),

$$h - f = \alpha_0 \ (T_h + \theta T_h^2) \text{ or } \alpha_0 = \frac{h - f}{T_h + \theta T_h^2} .$$

We may in general, without sensible error, assume that the value of one degree is uniform throughout the length of a single calibrating step, or if the column of mercury has been rather too long, we may subdivide the steps by interpolation. From the value of α_0, now obtained, we can find with sufficient exactness the temperature corresponding to the middle of the step f. It will now be convenient to make use of a table, derived from the values of A and B, showing the relative lengths of one degree at different temperatures on the supposition of uniform capacity of the tubes. The following are the values for every ten degrees, from $-70°$ to $+100°$ Fahr.:—

Temp. Fahr.	λ.	Temp. Fahr.	λ.
-70	0·831	$+20$	0·980
-60	0·848	30	0·997
-50	0·864	40	1·013
-40	0·881	50	1·030
-30	0·897	60	1·046
-20	0·914	70	1·063
-10	0·930	80	1·079
0	0·947	90	1·096
$+10$	0·964	100	1·112

The value in degrees of the step $f = \dfrac{1}{\alpha_f} = K_f$. Then calling the numbers in the table λ, since $\dfrac{\alpha_0}{\alpha_f} = \dfrac{\lambda_0}{\lambda_f}$, we find $K_f = \dfrac{1}{\alpha_0} \cdot \dfrac{\lambda_0}{\lambda_f} = \dfrac{K_0}{\lambda_f}$. This gives us the temperature corresponding to each end of the step f, and we may then proceed in like manner to find the values of the neighbouring steps, and so obtain successively the values throughout the whole range of the thermometer. The temperature corresponding to the point O in the figure is found by subtracting the sum of all the values of K between O and F from 32°. The length,

in turns of the dividing screw, for any degree x is $\frac{R_x}{K_x}$, where R is the length of the step in which x occurs, and K_x the equivalent number of degrees. A table can then be constructed, showing the lengths of each successive degree, commencing from the point O, by the aid of which the graduation may be performed. The scales extend to $-75°$ Fahr.

The time at my disposal was scarcely sufficient to test the thermometers supplied to the Arctic Expedition so completely as I should have wished. The mercurial thermometers were after their graduation compared incidentally at two or three different temperatures, and found to agree generally to $0°\cdot1$ Fahr. They were all placed in melting ice, when it was found that four of them read exactly $32°$, the other three, viz. Nos. 34, 46, 47, were about $0°\cdot1$ too low. In a few of these thermometers the column of mercury could be readily broken: when this column was moved to different portions of the scale, it was found to occupy precisely the same number of divisions. This was the case with four of the instruments; the other three not having been tested in this way.

The five spirit thermometers were compared at four different temperatures with a standard mercurial thermometer. The comparison at $0°$ being taken in ice and salt, is not very trustworthy. Their errors were as follows:—

Temp. by mer. stand.	S. 2.	S. 4.	S. 6.	S. 7.	S. 8.	Mean of errors.
65°	+0°·8	−0°·3	−0°·2	+1°·3	−0°·1	+0°·30
52	+0·8	−0·2	−0·3	+1·4	0·0	+0·34
32	+0·8	−0·1	−0·3	+1·4	−0·3	+0·30
0	+0·6	0·0	0·0	+1·7	+0·2	+0·50

The numbers in the column "Mean of errors" seem to indicate little error of a systematic nature. In the case of Nos. 2 and 7, the index error is very large: this, it is believed, is owing to some of the vapour of alcohol having become condensed in the upper portion of the tube before the fixed points were determined, and having escaped my notice; in fact the greatest attention is required to avoid errors from this source. These spirit thermometers cannot by any means be considered as standard, although they are doubtless more trustworthy than most of those usually made. The limited time at my command for the completion of the instruments, prevented the possibility of rectifying any blunders into which I might have fallen, owing to my inexperience in such work, and the intricacy of the problem.

JOHN WELSH.

Kew Observatory, April 21, 1852.

XLIX. *Intelligence and Miscellaneous Articles.*

APPARATUS FOR THE DETERMINATION OF CARBONIC ACID.
BY MAX. SCHAFFNER.

THIS is a modification of the apparatus of Will and Fresenius, which may be weighed on delicate balances, and admits of the determination of carbonic acid with very great accuracy, even when but small quantities of the substance to be examined can be employed. The figure shows it in natural size.

A is a tube into which the substance is introduced and weighed. It is then moistened with a little water, and the tube closed with a cork, *c*, through which pass two tubes, *g* and *r*, *g* passing through the cork *d* closing the tube B, which is filled with concentrated sulphuric acid as far as is shown in the figure. The tube *g* is closed by a piece of glass rod *s* fitting into a caoutchouc tube *k*. The tube *i* is to allow the escape of the carbonic acid disengaged. The whole apparatus may be hung on the balance by the wire hook *h*.

When thus arranged and weighed air is drawn through *i*, the acid rises through the tube *r* to the substance, and the carbonic acid disengaged is dried by passing through the sulphuric acid. When the effervescence has ceased, the glass rod *s* is taken out, and air drawn through the apparatus at *i* to remove all the carbonic acid. It is now weighed again, and the loss of weight gives the quantity of carbonic acid.—*Ann. der Chem. und Pharm.*, vol. lxxxii. p. 335.
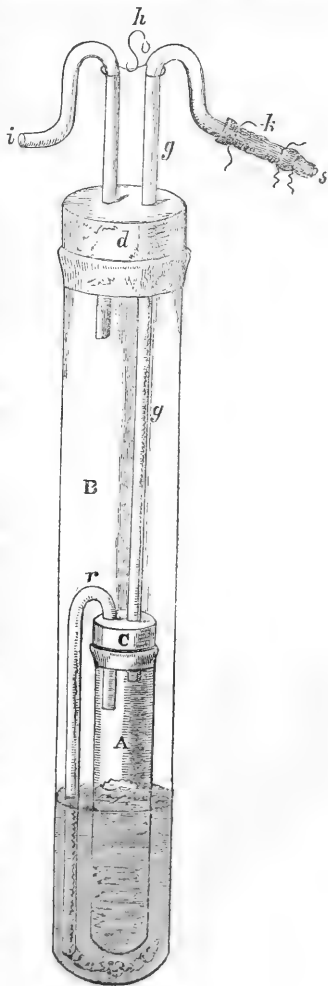
ON THE ABSORPTION OF HEAT BY A BISMUTH AND ANTIMONY JOINT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In writing his remarks on the "supposed absorption of heat by a bismuth and antimony joint," which occur in your last Number, Mr. Adie appears to have forgotten the experiment of Lenz, in which water was frozen at the point of junction, and the Centigrade thermometer sunk to 3·5 degrees below zero.

Queenwood College,   JOHN TYNDALL.

Sept. 21, 1852.

ON THE PEROXIDE OF SILVER.   BY F. MAHLA.

This substance, discovered by Ritter in 1804, is formed in a solution of nitrate of silver upon the positive wire of a galvanic couple, while crystals of metallic silver are deposited upon the negative wire. With the exception of the process described by Schönbein, by means of the still very mysterious substance called ozone, it cannot be obtained in any other way. Wallquist assigned to it the probable formula of AgO^2; but some time afterwards Fischer pointed out, that even when perfectly washed, this body gave off red nitrous vapours when heated, and consequently that this formula could not be correct. He moreover came to the conclusion that it contained both nitric acid and water, $4AgO^2 + AgO\,NO^5 + 2HO$. Mahla has recently examined this body in Wöhler's laboratory. He obtained it in the form of octohedral crystals, brilliant, but with curved planes, which were frequently grouped together in the direction of a principal axis in a prismatic form, and sometimes in narrow prisms an inch long, which appeared at first to belong to a quite different system. He never obtained it in tetrahedrons, which form it has been stated by Grotthuss to assume. The crystals are of a grayish-black colour, have a strong metallic lustre, and are very brittle. Specific gravity 5·474. Heated to 240° F. they evolve pure oxygen suddenly and with a kind of deflagration, at the same time falling to powder. Further heated, red vapours of nitrous acid are given off, and white metallic silver remains. Sulphuric and nitric acids convert it into ordinary silver salts of these acids with evolution of oxygen. It is dissolved by ammonia with violent evolution of nitrogen. It appeared possible that this body might consist of true peroxide of silver, which, in consequence of its rapid formation, had mechanically enclosed some nitrate of silver, to which the red vapours observed on heating it were owing. However, Fischer and L. Gmelin have shown that, even by the most careful washing, all the nitrate cannot be removed. Mahla has also found this to be the case; likewise that during the washing there is a feeble disengagement of oxygen; and he considers that the crystals are a true compound of AgO^2 with $AgO\,NO^5$.

On analysis he found it to contain in 100 parts,—

	Found.	Calculated.
Silver	81·176	81·097
Oxygen	16·030	16·023
Nitrogen	1·765	1·753
Water	1·166	1·127
	100·137	100·000

Assuming, as is most probable, that the nitrogen exists as nitric

acid, these numbers lead to the formula $5AgO^2 + AgO NO^5 + HO$. Such a compound is certainly without any known analogue.

Another mode of representing the constitution of this body is to suppose that it consists of nitrate of peroxide, $AgO^2 6NO^5$, and that the water is only a mechanical admixture. 100 parts should then contain—

Silver..............	81·19
Oxygen..............	17·05
Nitrogen	1·76

It may also be regarded as containing nitrous acid, NO^4, combined with peroxide and water, $6AgO^2 + NO^4 + HO$.

Mahla is, however, inclined to regard it as being essentially peroxide, containing mechanically mixed nitrate of silver and water; and the fact observed by Fischer, that when prepared with a solution of sulphate instead of nitrate of silver, the product contains sulphate of silver, supports this view, as does the observation made by Mahla, that fluoride of silver treated in the same way yields a body behaving in all respects like peroxide, and when heated evolving vapours which corrode glass strongly.—*Ann. der Chem. und Pharm.*, vol. lxxxii. p. 289.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1852.

Chiswick.—August 1. Very fine: uniformly overcast. 2. Very fine: densely clouded. 3. Partially overcast: slight shower. 4. Fine: clear. 5. Fine: slightly overcast. 6. Showers: heavy rain. 7. Cloudy: clear. 8. Fine: clear. 9, 10. Very fine. 11. Constant heavy rain. 12. Heavy rain: cloudy. 13. Very fine: clear. 14. Very fine: heavy rain at night. 15. Foggy: clear at night. 16. Heavy dew: fine: overcast. 17. Densely overcast: frequent vivid lightning and distant thunder: chiefly sheet lightning till 11 P.M., then forked lightning, with heavy thunder and rain in torrents. 18. Very fine: cloudy, with lightning at night. 19. Very fine: clear at night. 20. Overcast. 21. Light clouds: uniformly overcast. 22. Overcast: clear. 23. Cloudy and fine. 24. Overcast and mild: rain at night. 25. Cloudy and fine. 26. Slight haze: very fine: clear. 27. Very fine. 28. Foggy: remarkably dusky and dark about 9 A.M.: very fine. 29. Very fine: densely overcast: light clouds. 30. Cloudy: very fine: clear. 31. Clear: very fine.

Mean temperature of the month	....	63°·67
Mean temperature of Aug. 1851	....	62 ·79
Mean temperature of Aug. for the last twenty-six years	...	62 ·12
Average amount of rain in Aug.	....	2·44 inches.

Boston.—Aug. 1. Cloudy. 2. Fine. 3. Cloudy: rain P.M. 4. Fine. 5. Fine: rain P.M.: thunder and lightning. 6. Fine: rain P.M. 7, 8. Cloudy: rain P.M.: thunder and lightning. 9. Cloudy: rain P.M. 10. Fine: rain P.M.: thunder and lightning. 11. Rain: rain A.M. and P.M. 12. Cloudy: rain A.M. and P.M.: thunder and lightning. 13. Fine. 14. Fine: rain and hail P.M.: thunder and lightning. 15. Cloudy: rain A.M. 16. Fine. 17. Fine: rain P.M.: thunder and lightning. 18. Cloudy: rain early A.M. 19. Cloudy. 20. Cloudy: rain early A.M. 21—23. Cloudy. 24. Cloudy: rain A.M. and P.M. 25. Cloudy: rain A.M. 26, 27. Fine. 28. Cloudy: rain P.M. 29, 30. Fine. 31. Fine: rain and hail P.M.: thunder and lightning.

Sandwick Manse, Orkney.—Aug. 1, 2. Cloudy. 3. Cloudy: rain. 4. Bright: clear: fine. 5. Clear: fine: aurora. 6. Clear: fine. 7. Drops: clear: fine. 8. Bright: clear: fine. 9. Clear: fine. 10. Bright: fine: clear: fine. 11. Bright: fine: clear: fine. 12, 13. Bright: fine: cloudy: fine. 14. Bright: fine: clear: fine. 15. Clear: fine. 16. Cloudy: drops. 17. Fog: cloudy. 18. Drizzle: showers: clear. 19. Drizzle: bright: fine. 20. Showers: bright: fine. 21. Clear: fine: fog. 22. Fog: fine: fog. 23. Bright: fine: cloudy: fine: aurora. 24. Cloudy: fine: clear: fine. 25. Clear: fine: aurora. 26. Clear: fine: cloudy: fine. 27. Cloudy: fine. 28. Bright: fine: cloudy: fine. 29. Damp: clear: fine. 30, 31. Bright: fine: clear: fine.—This month has been very fine, warm and dry.

Mean temperature of Aug. for twenty-five years	....	54 ·75
Mean temperature of this month	....	60°·64
Average quantity of rain in Aug. for six years	....	3·05 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1852. Aug.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1.	30·037	29·937	29·46	29·90	29·88	82	52	73	60	58½	sw.	n.	wsw.			·16
2.	29·861	29·662	29·33	29·73	29·49	81	49	66	62½	58½	w.	calm	se.			
3.	29·464	29·268	28·95	29·25	29·20	71	47	72	61½	58	s.	s.	sse.	·03		·17
4.	29·419	29·338	28·83	29·15	29·28	72	46	60·5	61½	62	sw.	sw.	calm	·01		
5.	29·458	29·439	28·95	29·33	29·38	74	47	63	62	62	s.	s.		·04	·06	
6.	29·398	29·353	28·90	29·35	29·33	68	54	66	62	61	s.	s.	e.	·42		
7.	29·336	29·298	28·80	29·22	29·18	74	52	67	61	60	s.	sw.	se.	·12	·22	
☾ 8.	29·477	29·396	28·90	29·14	29·25	72	51	65	60	60	w.	sw.	calm	·02		·06
9.	29·523	29·499	29·00	29·34	29·39	71	47	67·5	61½	61	sw.	sw.	se.	·06	·34	
10.	29·631	29·583	29·08	29·46	29·52	71	49	67	62	60	sw.	sw.	calm	·28	·03	
11.	29·351	29·123	28·98	29·53	29·49	67	52	60·5	62	60	s.	sse.	n.	·78	·07	
12.	29·507	29·248	28·17	29·46	29·51	60	52	58	62	60	sw.	wnw.	n.	·47	·48	
13.	29·760	29·611	29·12	29·51	29·65	71	49	62	60½	60	nw.	wnw.	e.		·56	
14.	29·781	29·591	29·27	29·72	29·75	74	54	59·5	60	55	sw.	wnw.	e.	·64	·14	
15.	29·866	29·293	29·07	29·76	29·89	70	47	59	60	57	ne.	ne.	s.			
● 16.	29·930	29·917	29·47	29·85	29·79	71	62	64	60	59	se.	sse.	se.		1·25	
17.	29·914	29·678	29·37	29·62	29·48	76	59	69	61½	62	w.	s.	nw.	·76		
18.	29·862	29·741	29·23	29·69	29·84	74	59	63	65½	63	n.	wnw.	ne.		·67	
19.	29·930	29·909	29·40	30·04	30·10	72	55	65	63½	63	n.	n.			·09	
20.	30·074	29·991	29·50	30·05	30·12	70	57	60	61½	58½	w.	n.	se.	·03		
21.	30·205	30·128	29·64	30·33	30·31	64	60	66	58½	58½	ne.	n.	se.			
☽ 22.	30·242	30·213	29·73	30·23	30·18	72	53	67	59	61	n.	n.	s.	·03		
23.	30·260	30·195	29·75	30·23	30·18	72	55	62	60	59	sw.	ssw.	calm			·07
24.	30·168	29·995	29·60	29·84	29·87	71	55	70	56	60	sw.	sw.	calm			·01
25.	29·915	29·882	29·30	29·87	29·90	73	56	65·5	56	56	c.	se.	s.	·05	·19	·09
26.	30·045	29·973	29·52	29·99	30·05	74	54	59	62½	62½	e.	ene.	se.			·06
27.	30·117	30·107	29·65	30·03	29·97	75	54	63	62	62	sw.	calm	se.			
28.	30·040	29·993	29·57	29·84	29·88	76	57	64·5	61	61	se.	wsw.	se.			
○ 29.	30·024	29·969	29·47	29·77	29·77	76	47	65·5	58	58	sw.	wsw.	calm		·17	
30.	30·019	30·006	29·50	29·77	29·74	72	45	63·5	58	58	sw.	sw.	calm			
31.	30·077	30·007	29·50	29·69	29·74	69	44	57·5	57	57	sw.	w.	sw.			
Mean.	29·796	29·720	29·26	29·708	29·724	75·32	52·03	64·0	61·96	59·32				3·71	4·27	0·62

L. *On the Early History of Infinitesimals in England.*
By Professor De Morgan[*].

BEFORE stating the subject of this paper, I make the fol-
lowing addition to what appeared in June last, in proof
of Newton having written the account of the *Commerc. Epist.* in
Phil. Trans. No. 342. I there considered Pemberton as a wit-
ness, because, for reasons given, I could not imagine how Dr.
Wilson could have neglected to notice his dissent, if indeed he
had dissented: I considered that Pemberton had allowed Wilson,
in 1761, to affirm Newton's authorship. I have since found
that Pemberton also, and perhaps more distinctly, allowed Robins,
in 1735, to make the same affirmation. In the *Present State of
the Republic of Letters* for 1735 and 1736, and the *Works of the
Learned* for 1737, will be found more than twenty articles of the
Analyst[†] controversy, between Robins, at first anonymous,

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[*] Communicated by the Author.
[†] The most complete list I can give of the pieces of this controversy
may be made thus. To the twenty-six articles headed *Analyst Controversy*
in the catalogue of the Astronomical Society's library (1850) add the fol-
lowing. I. Walton, Vindication of Sir I. N's Fluxions, Dublin and London,
1735, 8vo.—I. Walton, The Catechism of the Author of the Minute Phi-
losopher fully answered, Dublin and London, 1735, 8vo.—A second edition
of the same, with an appendix in answer to the Reasons for not replying
to Mr. Walton's full answer, Dublin, 1735, 8vo.—Robins, Remarks on
Euler, Smith, and Jurin, Lond. 1739, 8vo.—Jurin, Reply to the last, Lond.
1739, 8vo.—Robins, Full confutation of the last, London, 1740, 8vo.—
Jurin, Letter to —— Esq. in answer to the last, London, 1741, 8vo.
From Walton's second edition it appears that there was a *third* tract by
Berkeley, entitled *Reasons for not replying to Mr. Walton's full answer*.
This I had never seen nor otherwise heard of: but I find that it has been

Dr. Jurin under the name of Philalethes Cantabrigiensis, and, finally, Pemberton himself. Now though Robins again and again affirms Newton's authorship, and though Pemberton appears in the controversy expressly to state his own personal knowledge of Newton's meaning and motives on certain points, and to support Robins in the view *he* had taken of them, yet Pemberton does not in any way contradict what Robins had advanced as to the authorship in question. Dr. Jurin, though not knowing on what foundation the paper in question is ascribed to Newton, affirms that, whether written by Newton or not, it was no doubt agreeable to his sentiments "as having been some years afterwards republish'd in Latin with his consent and approbation." (*Rep. of Lett.* July 1736, p. 51.) This addition is here made more for completeness* than for any need which there is of it : so far as I can judge, the conclusion of my last paper will remain undisputed.

My present subject is the early history of the *principle* of the differential calculus in England : I mean the principle of infinitely small quantities, as distinguished from that of prime and ultimate ratios or of limits. By the time the excitement of the great controversy had subsided, the continental mathematicians were well accustomed to the symbols of Leibnitz in connexion with the infinitesimal principle; the English to the symbols of Newton in connexion with both principles. But as to how the matter stood in England previous to the controversy, there has not been much inquiry. I here propose to collect notices of some points which the historian of fluxions will find to require his attention.

Up to the year 1704, and so far as algebraical calculus was concerned, Newton himself used infinitely small quantities; and nothing else in any document yet published. The prime and ultimate ratios, or limits, appear in the *Principia*, but are abandoned in those places in which fluxions are alluded to. I proceed to establish these assertions in detail.

In Newton's *earliest papers* published by Rigaud (*Hist. Essay on Princ.* App. pp. 20-24) the *velocities* are only differential coefficients : when A changes from x to $x + o$, B changes from y

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twice reprinted in the present century; first, in one edition of Berkeley's works published by Priestley, London, 1820, 3 vols. 8vo; secondly, in another, published by Tegg, London, 1843, 2 vols. 8vo, edited by G. N. Wright.

* It should be mentioned that at the time when Robins made the assertion, there was living a person who must have known the truth as well as Pemberton. I mean William Jones, the original possessor of Collins's papers, the first publisher of some of the most material, a member of the *Comm. Epist.* Committee, a personal friend of Newton, and the possessor of the best mathematical library of the day. Jones did not die till 1749.

to $y + oq \div p$, the velocities being p and q. Those terms in which o remains are "infinitely less" than those in which it is not, and are therefore "blotted out." And "those terms also vanish in which o still remains, because they are infinitely little."

The *Method of Fluxions*, translated by Colson* from Newton's Latin, and published in 1736, written, it is supposed, at some time in the period 1671–1676, is also strictly infinitesimal. The o is now the infinitely small increment of the time, and the fluxion, conceived and defined as a velocity, is its coefficient. Thus x, y, &c. become $x + \dot{x}o$, $y + \dot{y}o$, &c.: "but whereas o is supposed to be infinitely little, that it may represent the Moments of Quantities; the Terms that are multiply'd by it will be nothing in respect of the rest." (p. 25.)

In the first edition of the *Principia* (1687) the description of the fluxions is founded on infinitesimals, and in the second (1713) this foundation is somewhat altered. In the first, moments are *infinitely small quantities*; in the second, it is not clear what else they are. As in the following extract from the first edition, with its substitute in the second :—

First Ed. (Book ii. Lemma ii.).

"Cave tamen intellexeris particulas finitas. *Momenta, quam primum finitæ sunt magnitudinis, desinunt esse momenta. Finiri enim repugnat aliquatenus perpetuo eorum incremento vel decremento.* Intelligenda sunt principia jamjam nascentia finitarum magnitudinum."

Second Edition (ditto).

"Cave tamen intellexeris particulas finitas. *Particulæ finitæ non sunt momenta sed quantitates ipsæ ex momentis genitæ.* Intelligenda sunt principia jamjam nascentia finitarum magnitudinum."

Through the difficulty of the phrases in both extracts this much distinctly appears, that in the first edition the moments, or momentaneous increments, *are* infinitely small quantities: and this is what I assert. In the celebrated scholium which follows, the first edition states that Leibnitz's system hardly differed from Newton's except in words and symbols. The second edition adds *et Idea generationis quantitatum.* But this addition, as well as the alterations of phrase which are to support it, was made while the controversy was raging. It is only from a subsequent and private source that we can be well assured

* There is no doubt this work is Newton's: but, independently of internal evidence, if Colson were now charged with a fraud, there would be nothing to argue from, except the consent of Jones and Pemberton, as given by silence. The work, dedicated to the former, purports to be the one which would have been published by the latter, if Newton's death had not prevented.

of the meaning of the second edition, when we compare it with the first. Newton, writing to Keill in May, 1714, says "moments are infinitely little parts." And further, " wherever prickt letters represent moments and are without the letter *o*, this letter is always understood." (Edleston, p. 176.)

The treatise on the *Quadrature of Curves*, written* long before it was published, made its appearance, to a sufficient extent for our purpose, in 1693, in the Latin edition of Wallis's Algebra (*Op.* vol. ii. pp. 390–396). That this was substantially a contribution of Newton's, was obvious from the beginning: but it was not known, until Rigaud found it (*Hist. Essay on Princ.* p. 22), that a note made by Wallis in his own copy points out from p. 390 line 18 to p. 396 line 19 as being *Newton's own words* [the first word of all, *clarissimus*, no doubt excepted, as also the parenthetic description of David Gregory, *nunc meus collega dignissimus*]. The famous proposition *Data æquatione quotcunque* &c. occurs in this extract; and is of course repeated in the *Quadratura Curvarum*, at the end of the Optics, in 1704. The two publications of the proposition agree sentence for sentence, and clause for clause, but not word for word. The following comparisons will prove my assertion, both as to the first adoption, and subsequent abandonment, of uncloaked infinitesimals.

1693.	1704.
" quantitas infinite parva . . . . Et hæ quantitates proximo temporis momento per accessum incrementorum momentanorum evadent $z + o\dot{z}$	"quantitas admodum parva. . . Et si quantitates fluentes jam sunt z, y, et x, hæ post momentum temporis incrementis suis $o\dot{z}, o\dot{y}, o\dot{x}$ auctæ, evadent $z + o\dot{z}$ . . . .
" Terminos multiplicatos per *o* tanquam infinite parvos dele, et manebit æquatio . . . ."	" Minuatur quantitas *o* in infinitum, et neglectis terminis evanescentibus restabit. . . ."

It will now, I think, be very clear that Newton commenced with the infinitesimal system in as absolute a form as did Leibnitz, so far as infinitely small quantities of the first order are concerned. Further than these he did not go; and the early distinction between the systems of the two is this, that Newton, holding to the conception of the *velocity* or *fluxion*, used the infinitely small† increment as a means of determining it; while,

* " The book of Quadratures is ancient, many things being cited out of it by me in my Letter of 24 Octob 1676." (Newton to Keill; Edleston, p. 176.)

† It is also to be noticed that Leibnitz and the Bernoullis demand the method of exhaustions, or something equivalent, whenever an objection is raised to infinitesimals. They do not face a human enemy with small shot; they only use it to kill game.

with Leibnitz, the relation of the infinitely small increments is itself the object of determination. That the difference between the two did rest mainly upon a difference in the *idea generationis quantitatum* is perfectly true : the fault to be found with the alteration of the scholium is that the reader is left to infer that the difference of the ideas of generation of quantity was as visible to Newton as the difference of symbols, from the moment when he received Leibnitz's communication. This might easily have escaped notice in making the alteration. The fluxional idea was not, so far as I can find, in the mind of Cavalieri, Fermat, or Leibnitz : I shall have occasion elsewhere to notice its occurrence among the schoolmen.

Before 1693, no one could know anything of Newton's fluxions except from private communication. Before 1704, no one, except in the same way, could know that Newton preferred the method of limits to that of infinitesimals, in algebraical *calculus*.

Of the English contemporaries of Newton, the first who wrote on the differential calculus of Leibnitz was John Craig. This writer, though in one* respect of absurd memory, had original power, and capacity for assimilating the various apparently different systems of the day. His earliest work was *Methodus Figurarum . . . . Quadraturas determinandi,* London, 1685, 4to (pp. iv + 43 + i(plate)). Some additions are made to this tract in the *Phil. Trans.* Nos. 183 and 232. In the preface of 1718 (presently mentioned), Craig informs us that in 1685 he was a resident† at Cambridge, and that Newton, at his request, read his writing before it was sent to press. Here, however, we shall see reason to think that he spoke of the wrong tract. After some exemplifications of Barrow and Sluse—not referring to Newton as having any method of quadratures, but only as to the binomial theorem—he proceeds to say that nothing is wanting to extend his method to all but transcendental curves, except only the removal of two difficulties. The first difficulty is the extraction of roots, which he gets over by a series of Newton's, which he hears that Dr. Wallis has sent to press, but which New-

* In 1699 he published his *Theologiæ Christianæ Principia Mathematica,* an ill-judged imitation of Newton's title, in which he calculates that the evidence of Christianity will be reduced to nothing by lapse of time in A.D. 3150, at which time therefore a renewal of revelation will take place. Craig is now better known by this tract (which is said to have been republished and answered in Germany as late as 1755) than by his other writings.

† *Cantabrigiæ commoratus* means, I suppose, that he was a member of the University ; but I cannot find his name in the list of graduates. He was afterwards a clergyman in Dorsetshire, and was a Scotchman by birth. His diocesan was Burnet, to whom he expresses unusual obligation : and Burnet's son, who was afterwards a member of the *Comm. Epist.* Committee, was his pupil. I cannot find the date of his death, but by the list of subscribers to De Moivre's *Misc. Analyt.* I see that he was alive in 1730.

ton has had the goodness to communicate in manuscript. This assertion corroborates itself, for Craig does use the binomial theorem, which at the time he was writing had not been in print : Wallis's Algebra was not published till the very end of 1684. His account of the second difficulty is as follows :—

"The second difficulty is when the value of the ordinate has irrational terms [*asymmetris*] ; for analysts well know that it is very laborious to free an equation from irrationality of more than four terms.   But a most excellent remedy has been applied by the celebrated geometer G. G. Leibnitz in his new method of finding tangents published in the *Acta Eruditorum* of last year : in which an easy method is shown of finding tangents without removing the irrational terms, be they ever so much involved in the equation."   Craig then proceeds to use the differential calculus under the symbols of Leibnitz, but with some elementary mistakes.   We see here the singular indifference which Newton at that time, and long afterwards, showed towards his own calculus.   It appears that when he communicated to Craig, for help in the quadrature of curves, his binomial theorem, at the very period when Leibnitz had just announced the differential calculus, he never gave a hint that he himself had had long possession of a similar method, and had exchanged communications with Leibnitz on the subject eight years before.

The second of Craig's separate tracts is *Tractatus Mathematicus de Figurarum Curvilinearum Quadraturis et Locis Geometricis,* London, 1693, 4to (pp. iv + 76 and plate).   That *this* was the tract which Newton examined before it was printed, I infer as follows. In the preface of 1718, before mentioned, Craig says that Newton proposed two curves, of which he gives the equations, as examples in corroboration of Craig's objections against D. T. (Tschirnhauss).   Now the attack upon D. T. is at the end of this second tract, and the curves specified are the first two examples at the beginning.   Moreover, in the *first* pamphlet Craig was no deeper in the differential calculus than to imagine that $Pdy = Qdx$ always gives $Py = Qx$, which we may undertake to say Newton could not have passed without detection, if he had seen the manuscript, even though he had only given it a glance.

It is also to be noticed that in the second tract the name of Newton does not occur once, though it is full of the differential calculus, and Leibnitz, Sluse, Barrow, Gregory, &c. are frequently mentioned.   This, under all the circumstances, we may suspect was Newton's own doing.   And I am strongly inclined to think that it was this very tract of Craig's which immediately suggested to Newton the progress which the views of Leibnitz were making, and induced him to forward to Wallis the extracts from the *Quadr. Curv.* which I have already mentioned.

My copy of Craig's second tract has written on the title-page " Is. Walton Donum Authoris. Jun : 7. 1693." The form of this inscription, especially the date, shows that it was made by the receiver, and not by the giver. Supposing this receiver to be I. Walton, who afterwards took part in the *Analyst* controversy, the book was in Ireland on the 7th of June, and had most probably been published many weeks before. The *imprimatur* of Wallis's second volume is dated August 28, and the preface July 12. The volume has 880 pages; the printing began, as the preface informs us, in 1692, and Newton's contribution (a most evident interpolation) is at page 390. If Craig's tract were seen by Newton, as would seem most likely, at the end of 1692, an immediate communication to Wallis would have arrived in sufficient time to appear where it actually does. Further, Craig is mentioned in Newton's contribution as having written to Newton for a series which was to be communicated to David Gregory. Now Craig, in the preface of 1718, gives an account of this correspondence, as some months posterior to the examination of his manuscript by Newton; consequently, Newton's contribution to Wallis is posterior to his having seen Craig's manuscript. I conclude, then, that Newton, seeing the progress the differential calculus was likely to make in England, procured the entire suppression of his own name in Craig's tract, and made up his mind to insert a part of his own treatise in the forthcoming work of Wallis. This will explain why Craig did not *then* mention either Newton's examination of the manuscript, or his supply of examples : the tone in which he attacks Tschirnhauss is so acrimonious, that we may be sure Newton would have desired not to appear, even indirectly. It is true that Newton (Edleston, p. 176) speaks of the contribution as sent in 1692, at Wallis's request. That it must have been the very end of 1692, which is quite consistent with my suppositions, appears from this, that the contribution itself, or a parenthesis of Wallis, refers to previous letters of August and September, from Newton to Wallis, apparently on series.

Craig continued to write in the *Philosophical Transactions,* and he uses the differential calculus in 1701 in No. 268, in 1703 in No. 284, in 1704 in No. 289, and in 1708 in No. 314. In the second of these papers, he treats the differential calculus as universally established : *ut omnibus notum,* $zy - \int : ydz = \int : zdy$. He also wrote one or more papers in the Leipsic acts. His third separate publication followed the great controversy : it is *De calculo fluentium libri duo. Quibus subjunguntur libri duo de optica analytica.* London, 1718, 4to (pp. viii + 92). We have now nothing but fluxions; not a word of the •differential calculus :

except slightly, in one scholium, the name of Leibnitz does not appear.

De Moivre took his idea of fluxions direct from the *Principia*, and (*Phil. Trans.* 1695, No. 216) uses the infinitely small moments. His fluxion of an area, for instance, is an infinitely small rectangle. Halley, in his paper on logarithms in the same number, uses infinitely small *ratiunculæ* and *differentiolæ* in a manner which must have astounded every beginner who ever saw the reprint in Sherwin's Logarithms.

Of the elementary writers, Harris and Hayes (1702 and 1704), I have spoken in the *Companion to the Almanac* for 1852 (p. 15). Both used infinitely small quantities. Hayes adopts infinitesimals of infinitesimals : and as he happened to publish his work in the very year in which Newton declared himself against all infinitely small quantities, his book was neglected, in spite of its merit. His list of the writers to whom he is indebted includes Wallis, Barrow, *Newton, Leibnitz, De L'Hôpital, the Bernoullis, Craig,* Cheyne, Gregory, Tschirnhauss, De Moivre, Fatio, Varignon, Nieuwentiit, Carré : those in Italics being those to whom he considers himself particularly indebted.

Cotes, in 1701 (Edleston, p. 196), makes the term fluxion interchangeable with *differentiola,* and considers it as infinitely small. Cotes was then an undergraduate, and, as is pretty clear from his enumeration of results, a reader of transactions and other original papers, and also, perhaps, of De L'Hôpital.

Cheyne, in his *Fluxionum Methodus inversa,* London, 1703, 4to (pp. iv + 128), refers to and follows Newton's contribution to Wallis. By such suppositions as $\dot{x} = 1$, he shows that he interprets this symbol, taken apart from o, in the same manner as Newton. There is an additional leaf, published in 1704, entitled *Addenda et adnotanda in libro Georgii Cheynæi,* which I have never seen. De Moivre in his *Animadversiones in D. Georgii Cheynæi Tractatum* . . . . London, 1704, 8vo (pp. xiv + 129) follows the plan of Cheyne.

Lastly, Fatio de Duillier, in his memorable paper on the line of shortest descent, *Lineæ brevissimi descensus investigatio* . . . . London, 1699, 4to (pp. 24 and plate), uses fluxions as infinitely small quantities.

Thus it appears that, up to 1704, all the writers who used the new calculus, used infinitely small quantities; that all who used $\dot{x}$, except Newton and Cheyne, interpreted it as an infinitely small quantity; and that Newton himself, though he never varied in his meaning of $\dot{x}$, used $\dot{x}o$ and not $\dot{x}$, admitting that he sometimes omitted o, which has the force of dt.

In 1704, Newton, in the *Quadratura Curvarum,* renounced and abjured the infinitely small quantity; but he did it in a

manner which would lead any one to suppose that he had never held it. In the new preface, written about 1704, he says, *Quantitates Mathematicas non ut ex partibus quam minimis constantes, sed ut motu continuo descriptas hic considero* : and again, *Errores quam minimi in rebus mathematicis non sunt contemnendi*. So far we have nothing more than *verba de præsenti* : not so, however, in *incidi paulatim Annis* 1665 *et* 1666 *in Methodum Fluxionum* qua hic usus sum *in Quadratura Curvarum*. And yet there is something like a recognition of *some one* having used infinitely small quantities in *Fluxions*, contained in the following words : *volui ostendere quod in Methodo Fluxionum non opus sit figuras infinite parvas in Geometriam introducere* : nothing is wanted except an avowal that the *some one* was Newton himself. The want of this avowal was afterwards a rock of offence. Berkeley, in the *Analyst*, could not or would* not see that Newton of 1687 and Newton of 1704 were of two different modes of thought. He arrays the infinitely small moments of the *Principia*, and their rejection in comparison of finite quantities, against the declaration of the *Quadratura Curvarum*, that the smallest possible errors must not be neglected. The defenders of Newton got over this palpable contradiction in every way but the true one, namely, the avowal of a change of system.

Further, we see a return to the allowance of infinitely small quantities in 1713, in the second edition of the *Principia*. Before I noticed the letter of May 1714, quoted in a preceding page, all I could do was to feel thankful that the meaning of a *moment* in this second edition was not to be settled by me. After the declaration of 1704, and the changes of language in the scholium, I could but suppose that the *principia nascentia* of the second edition were not those of the first : nevertheless, what they could be except infinitely small, I had no power to imagine : I have now made it clear that infinitely small quantities were still retained. I may notice in passing, that there is something like an impression among us that infinitesimals are repugnant to the English taste and mode of thought, and that from the beginning they were looked upon with dislike and suspicion. This it fully appears was not the case : no mathematical novelty ever found a readier acceptance among us.

* Dishonesty must never be insinuated of Berkeley. But the Analyst was intentionally a publication involving the principle of Dr. Whateley's argument against the existence of Buonaparte; and Berkeley was strictly to take what he found. The Analyst is a tract which could not have been written except by a person who knew how to answer it. But it is singular that Berkeley, though he makes his fictitious character nearly as clear as afterwards did Whateley, has generally been treated as a real opponent of fluxions. Let us hope that the arch Archbishop will fare better than the arch Bishop.

The neglect of the early history of fluxions which has prevailed in this country, except only as to the controversy, is well illustrated by the state of the Royal Society's library, as it appears in the catalogue of 1839. There will be found wanting the *Analyst* (except in Berkeley's works of 1820), and most of the tracts which followed it, all the works of Craig, De Moivre's answer to Cheyne, the first edition of Harris, the folio of Hayes, and even the very celebrated tract of Fatio de Duillier, though it is one of the documents of the controversy with which the Royal Society was afterwards especially concerned.

October 2, 1852.

LI. *On a new Aspirator.*
By Thomas Andrews, *M.D., F.R.S., M.R.I.A.*[*]

[With a Plate.]

IN this aspirator the current of air is produced by raising a cylindrical vessel A (Plate III.), open at bottom and immersed in water contained in the outer vessel B. The tube C communicates with the inner and upper part of A. The cords gg are attached to weights which counterpoise A. In short, the construction is precisely the same as that of the gasometers which are used in the preparation of gas for illuminating purposes on the large scale. In order to raise A, it is connected by means of the brass rod fh with the free end of the chain of a common one-day German clock, whose weight, k, is increased so as to enable it both to move the machinery of the clock and also to overcome the resistance of A. By this means the cylinder A is elevated at a perfectly uniform rate, and which may be varied at pleasure by augmenting or diminishing the length of the clock's pendulum. When the cylinder has attained the proper height, its motion is arrested by the nut f, the clock being at the same time stopped. To prepare for a second operation, the clock weight g is removed, the stopcock d closed and e opened; and by applying a gentle pressure to the top of A, or laying a small weight upon it, A is made to descend till the lower edge rests upon B. t is a thermometer having its bulb inserted in A.

It is very easy to determine by experiment the precise volume of air which enters the receiver A during its ascent; and if the apparatus be carefully constructed, no appreciable error will arise from inequalities in the volume of air entering A in different observations. And knowing the height of the barometer and the temperature of the air in A, which gives at the same time the tension of the aqueous vapour, it is easy to calculate the volume

of air at a standard temperature and pressure which has passed through the apparatus. If the aspirator be applied to the determination of the amount of aqueous vapour, or other absorbable constituent in the atmosphere, it will be necessary to find also the mean temperature and pressure of the air during the course of the experiment. With these data, a simple calculation will give the exact weight or volume of the aqueous vapour in the air.

It is unnecessary to point out the many applications which an aspirator affording an absolutely uniform current of air may receive. In the chemical laboratory it will frequently be found a very convenient instrument of research, and may even be applied in some cases to quantitative experiments. But it is chiefly in atmospheric inquiries that this aspirator will find its applications; and in the determination of the amount of oxygen, carbonic acid, aqueous vapour, and even ammonia in the air, it will prove, if I am not mistaken, a useful addition to the meteorological observatory. For these various objects its size and form will require to be modified; and it will be necessary where large quantities of air are operated on, to enlarge considerably its dimensions and employ more powerful clock-machinery than I have found necessary.

The capacity of A in the apparatus I employed was 21·623 litres, or nearly 1320 cubic inches. With a pendulum of the ordinary length, six hours were required for its ascent; but by reducing the length of the pendulum, the same operation was completed in an hour and a half. In the latter case, a current of air at the rate of 240 cent. cub. (14·6 cub. in.) per minute passed through the apparatus, and under these conditions I made a few experiments on the relative desiccating powers of certain substances, the results of which I will now very briefly state.

With carefully dried gypsum in T, and fragments of pumice moistened with sulphuric acid in T′, the latter underwent no change of weight after the passage of the first measure of the aspirator; but in the next experiment (the same tubes being employed) it gained 0·056 grm. (0·86 grain); and in a subsequent trial, after two hours had been allowed to elapse in order to allow the moisture to be imbibed by the gypsum, there was still a gain of 0·034 grm. (0·52 gr.).

With fused chloride of calcium in T, and sulphuric acid in T′, the gains of T′ in three consecutive experiments were respectively 0·033, 0·040, and 0·040 grm.

Dry sulphate of lime appears therefore to be superior to fused chloride of calcium, but neither desiccates the air with sufficient energy to be employed in these experiments.

With sulphuric acid in T as well as in T′, the latter experienced no change of weight till sixteen measures of the aspirator

had passed through the tubes, but afterwards it began rapidly to increase in weight.

With well-dried, but not fused, chloride of calcium, as recommended long ago by Liebig for organic analysis, in T, and sulphuric acid in T', some interesting results were obtained. To ascertain whether the absorption of moisture by T' was complete, a third tube containing sulphuric acid was placed between it and the aspirator. Sixty measures of the aspirator were passed in succession through this series of tubes, which were only removed from time to time for the purpose of being weighed. The gain of tube T after this operation amounted to no less than 12·252 grms. (189·08 grs.) ; that of T' to 0·141 grm. (2·17 grs.) ; and of the tube next the aspirator to 0·21 grm. (0·32 gr.). It is important to remark that the gain of T' was a uniform quantity from the beginning to the end of the experiment; its average increase of weight for each elevation of the aspirator from the first to the twenty-fifth time being 0·0024 grm. (0·037 gr.), and from the twenty-fifth to the sixtieth time 0·0023 grm. (0·035 gr.). The gain of the third tube is quite insignificant, not amounting to $\frac{1}{600}$dth part of the whole quantity of aqueous vapour, and probably arising from moisture derived from the air in the aspirator, or from some accidental cause. The experiment in this case was continued till the stream of air was arrested by the liquefaction of the chloride of calcium in the further end of T from the aspirator; yet the desiccating power of the chloride of calcium in the other limb continued unimpaired till the end. It appears, then, that the whole of the moisture may be absorbed from 1296 litres (or nearly 80,000 cubic inches) of atmospheric air in its ordinary state in this country by means of a chloride of calcium tube weighing about 90 grms. (1400 grs.), aided by a supplementary tube containing sulphuric acid, and of about half the weight. As these tubes may be placed at the same time on the balance, no additional trouble is incurred by employing them both. Further experiments are, however, required to determine whether the small increase of weight sustained by the sulphuric acid tube arose actually from the absorption of aqueous vapour, or whether it may not have been due to the absorption by the sulphuric acid of a part of the carbonic acid existing in the air*. If this latter view prove to be correct, dry chloride of calcium should be substituted for sulphuric acid in the tube T.

A slight modification of the apparatus would give the quantity of moisture in the air for shorter intervals of time; and there can be no doubt that the tubes would continue to absorb all the moisture from air passing with much greater velocity than

* See the observations on this subject of Prof. Rogers in the Chemical Gazette, vol. vii. p. 477.

in the experiment just described. By increasing, if necessary, the length of the U-tubes, it would not be difficult to collect from 0·5 to 1 grm. (7 to 14 grains) of water from the air in periods of an hour, or even half an hour; and thus the amount of aqueous vapour might be determined accurately to $\frac{1}{500}$dth part of the whole quantity, instead of an uncertain approximation to $\frac{1}{5}$th or $\frac{1}{10}$th of the same, which is perhaps all that can be attained by the methods now generally in use.

Queen's College, Belfast,
  September 28, 1852.

LII. *On the Occurrence of Metallic Iron in Fossil Wood.* By W. G. LETTSOM, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,   Madrid, Sept. 23, 1852.

IN the recent edition of Phillips's Mineralogy by Messrs. Brooke and Miller, at page 685 mention is made of the discovery of metallic iron in certain metamorphic rocks in Antrim by Dr. Andrews of Belfast; and as this fact is a novel one, perhaps the following account of the occurrence of this substance in a metallic state in Sweden, translated from a periodical which appears here under the title of *El Restaurador Farmeceutico*, may be thought worthy of a place in the pages of the Philosophical Magazine.

M. Bahr, a pupil of Prof. Svanberg, has had occasion to analyse the fossil wood derived from an island in the lake of Ralang in Smaland, whose composition resembled that of the mineral known by the name of limonite.

On endeavouring to pulverize this mineral in small quantities, he observed some minute tenacious grains which yielded but very little to the action of the agate pestle. These grains were simply metallic iron; they became flattened by the blow of a hammer, were attracted by a magnet, and were soluble in acid with evolution of hydrogen.

M. Bahr enters upon the question, whether this iron had been formed in the mass of this wood by the reduction of some soluble salt of iron, or whether it has been originally introduced in the form of a nail, or the fragment of some tool or instrument, which, after having been partially destroyed by oxidation, may nevertheless have left some traces of its presence in the metallic state.

After having examined and discussed with care the origin, characters, structure, and composition of the specimen of fossil wood in question, M. Bahr arrives at the conclusion that the

iron has been really deposited in the metallic state, and that the singular mineral submitted to his examination presents the first well-established instance of the occurrence of terrestrial native iron.

Among the points on which M. Bahr bases his opinion, may be cited the following :—

The specimen of fossil wood in question was detached on the 28th of August 1798 from the thick end of the trunk of a tree that was met with on the floating island in the lake of Ralang in Smaland. This island, after being submerged for four years, had risen to the surface the evening before. At the end of the trunk there were two plates of copper, bearing an inscription commemorative of the date when Gustavus III. visited this singular island, which appears in its origin to have formed a tongue of land boarded over, which became detached from the mainland by the action of the waters of the lake. It is usually submerged, but from time to time it rises to the surface for a few days, most commonly in the months of August and September.

M. Bahr had submitted to him two specimens of this fossil wood, which in its external appearance very much resembled a fragment of limonite. The specific gravity of one of these fragments was 3·85, of the other only 2·94. When the surface was brushed with the feathery part of a quill, metallic iron was rendered visible, penetrating the general mass of the wood in a pulverulent form, or in the state of scales and angular and rounded grains. This structure was more clearly seen by means of a magnifying glass or of a microscope, which showed the fibres of the wood separated by the pulverulent iron. Here and there these grains of iron were of an appreciable magnitude, and were joined together like beads in a rosary. A group of five such grains was about a millimetre in length. In other portions of the specimen they might be detached, leaving cavities resembling the organic cellules. The specific gravity of this iron (in consequence, probably, of its admixture with organic matters) is very inconsiderable. In two trials it was found to be 6·248 and 6·4972 ; in a third and last experiment instituted with a grain of iron flattened by the blow of a hammer, it was found to be 6·6255.

M. Bahr analysed these grains of iron, having first freed them as much as possible from all organic substances, and the results that he obtained are stated below. The solution was effected by means of very dilute nitric acid, and the portion thus dissolved amounted to 80·16 per cent. of the total weight of the grains, and contained the following substances calculated in the form of oxides :—

Silica . . . . 0·818
Phosphoric acid . . . . . . 0·500
Nickel (and cobalt) . . . . 0·129
Lime and magnesia . . . . 0·178
Alumina . . . . 0·213
Vanadic acid . . . . trace
Manganese . . . . trace
Protoxide of iron (by difference) 98·171
_____
100·009

The residue, insoluble in very dilute nitric acid, contained also portions which obeyed the action of the magnet; and these in like manner were subjected to analysis, and gave the following results calculated in the state of oxides :—

Silica . . . . 0·619
Phosphoric acid . . . . . . 3·159
Vanadic acid . . . . 1·402
Oxide of nickel (with oxide of cobalt) 0·737
Oxide of iron . . . . 94·464
Manganese . . . . trace
_____
100·381

So far M. Bahr, the original source of whose paper I am unable to state, as it is not given in the Spanish version of it. In conclusion I may observe, that I possess in my collection of minerals a specimen of limonite from Passau in Bavaria containing small threads and veins of iron in the metallic state.

I am, Gentlemen,

Your obedient Servant,

W. G. LETTSOM.

LIII. *On* Staudt's *Theorems concerning the contents of Polygons and Polyhedrons, with a Note on a new and resembling Class of Theorems.* By J. J. SYLVESTER, *Barrister-at-Law* [*].

THE beautiful and important geometrical theorems of Staudt are, I believe, little, if at all, known to English mathematicians. They originally appeared in Crelle's Journal for the year 1843, and have been recently reproduced in M. Terquem's *Nouvelles Annales* for the August Number of the present year.

These theorems may be summed up, in a word, as intended to show the possibility and method of expressing the product of any two polygons or any two polyhedrons as entire functions of the squares of the distances of the angular points of the two figures from one another. The well-known expression for the square of

[*] Communicated by the Author.

the area of a triangle in terms of the sides (in which, when expanded, only even powers of the lengths of the sides appear), is but a particular case of Staudt's theorem for polygons, for it may be considered as the case of two equal and similar triangles whose angular points coincide. So in like manner, as observed by Staudt, a similar expression in terms of its sides may be found for the square of a pyramid. This expression had, however, been previously given (although, by a strange negligence, not named for what it was) by Mr. Cayley in the Cambridge Mathematical Journal for the year 1841 *, in his paper on the relations between the mutual distances to one another of four points in a plane and five points in space; the singularly ingenious (and as singularly undisclosed) principle of that paper consisting in obtaining an expression for the volume of a pyramid in terms of its sides, and equating this, or rather its square, to zero as the conditions of the four angular points lying in the same plane.

The analogous condition for five points in space is virtually deduced by going out into rational space of four dimensions, and equating to zero the expression obtained for the volume of a plupyramid; meaning thereby the figure which stands in the same relation to space of four as a pyramid to space of three dimensions. Mr. Cayley's method, if it had been pursued a step further, would have led him to a complete anticipation of the principal part of Staudt's discovery. The method here given is not substantially different from Mr. Cayley's, but is made to rest upon a more general principle of transformation than that which he has employed. As to Staudt's own method, it is as clumsy and circuitous as his results are simple and beautiful. Geometry, trigonometry and statics, are laid under contribution to demonstrate relations which will be seen to flow as immediate and obvious consequences from the most elementary principles in the algorithm of determinants. Perhaps, however, M. Staudt's method is as good as could be found in the absence of the application of the method of determinants, the powers of which, even so recently as ten years ago, were not so well understood or so freely applied as at the present day.

The following new but simple theorem, of which I shall have occasion to make use, will be found to be a very useful addition to the ordinary method for the multiplication of determinants. " If the determinants represented by two square matrices are to be multiplied together, *any number of columns may be cut off* from the one matrix, and a corresponding number of columns

---

* Quære, Is not this expression for the volume of a pyramid in terms of its sides to be found in some previous writer? It can hardly have escaped inquiry.

from the other. Each of the lines in either one of the matrices
so reduced in width as aforesaid being then multiplied by each
line of the other, and the results of the multiplication arranged
as a square matrix and bordered with the two respective sets of
columns cut off arranged symmetrically (the one set parallel to
the new columns, the other set parallel to the new lines), the
complete determinant represented by the new matrix so bordered
(abstraction made of the algebraical sign) will be the product of
the two original determinants."

Thus $\begin{matrix} a & b \\ c & d \end{matrix} \times \begin{matrix} \alpha & \beta \\ \gamma & \delta \end{matrix}$ may be put under any one of the three fol-
lowing forms:—

$$a\alpha+b\beta \; ; \; a\gamma+b\delta$$
$$c\alpha+d\beta \; ; \; c\gamma+d\delta,$$

or

$$\begin{matrix} a\alpha; & a\gamma; & b \\ c\alpha; & c\gamma; & d \\ \beta; & \delta; & 0 \end{matrix} \quad \text{or} \quad \begin{matrix} 2; & 2; & a; & b\,* \\ 2; & 2; & c; & d \\ \alpha; & \beta; & 0; & 0 \\ \gamma; & \delta; & 0; & 0. \end{matrix}$$

And in general for two matrices of n^2 terms each, this rule of
multiplication will give $(n+1)$ distinct forms representing their
products.

Thus, as a further example,

$$\begin{matrix} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{matrix} \quad \times \quad \begin{matrix} \alpha & \beta & \gamma \\ \alpha' & \beta' & \gamma' \\ \alpha'' & \beta'' & \gamma'' \end{matrix} ;$$

besides, the first and last form will be representable by the two
intermediate forms

$$-\left\{\begin{matrix} a\alpha+b\beta & a\alpha'+b\beta' & a\alpha''+b\beta'' & c \\ a'\alpha+b'\beta & a'\alpha'+b'\beta' & a'\alpha''+b'\beta'' & c' \\ a''\alpha+b''\beta & a''\alpha'+b''\beta' & a''\alpha''+b''\beta'' & c'' \\ \gamma & \gamma' & \gamma'' & 0 \end{matrix}\right\}$$

and

$$+\left\{\begin{matrix} a\alpha; & a\alpha'; & a\alpha''; & b; & c \\ a'\alpha; & a'\alpha'; & a'\alpha''; & b'; & c' \\ a''\alpha; & a''\alpha'; & a''\alpha''; & b''; & c'' \\ \beta; & \beta'; & \beta''; & 0; & 0 \\ \gamma; & \gamma'; & \gamma''; & 0; & 0 \end{matrix}\right\}$$

* Any quantities might be substituted instead of 2 in the places occupied
by the figure in the above determinant, as such terms do not influence the
result; this figure is probably, however, the proper quantity arising from
the application of the rule, because (as all who have calculated with deter-
minants are aware) the value of the determinant represented by a matrix
of *no places* is not zero but unity.

To arrive, for instance, at the latter of these two forms, we have only to write the two given matrices under the respective forms

$$
\begin{array}{ccccc}
a & b & c & 0 & 0 \\
a' & b' & c' & 0 & 0 \\
a'' & b'' & c'' & 0 & 0 \\
0 & 0 & 0 & 1 & 0 \\
0 & 0 & 0 & 0 & 1
\end{array}
\qquad
\begin{array}{ccccc}
\alpha & 0 & 0 & \beta & \gamma \\
\alpha' & 0 & 0 & \beta' & \gamma' \\
\alpha'' & 0 & 0 & \beta'' & \gamma'' \\
0 & 1 & 0 & 0 & 0 \\
0 & 0 & 1 & 0 & 0
\end{array}
$$

and then apply the ordinary rule of multiplication. So, again, to arrive at the first of the above written two forms, we must write the two given matrices under the respective forms

$$
\begin{array}{cccc}
a & b & c & 0 \\
a' & b' & c' & 0 \\
a'' & b'' & c'' & 0 \\
0 & 0 & 0 & 1
\end{array}
\quad\text{and} -
\begin{array}{cccc}
\alpha & \beta & 0 & c \\
\alpha' & \beta' & 0 & c' \\
\alpha'' & \beta'' & 0 & \gamma'' \\
0 & 0 & 1 & 0
\end{array}
$$

and proceed as before.

This rule is interesting as exhibiting, as above shown, a complete scale whereby we may descend from the ordinary mode of representing the product of two determinants to the form, also known, where the two original determinants are made to occupy opposite quadrants of a square whose places in one of the remaining quadrants are left vacant, and shows us that under one aspect at least this latter form may be regarded as a matrix *bordered* by the two given matrices.

A second but obvious theorem requiring preliminary notice is the following, viz. that the value of the determinant to the matrix

$$
\begin{array}{ccccc}
a_{1,1}; & a_{1,2}; & \ldots & a_{1,n}; & 1 \\
a_{2,1}; & a_{1,2}; & \ldots & a_{2,n}; & 1 \\
\cdot & \cdot & & \cdot & \cdot \\
a_{n,1}; & a_{n,2}; & \ldots & a_{n,n}; & 1 \\
1; & 1; & \ldots & 1; & 0
\end{array}
$$

is the same as the value of the determinant to the matrix

$$
\begin{array}{ccccc}
A_{1,1}; & A_{1,2}; & \ldots & A_{1,n}; & 1 \\
A_{2,1}; & A_{2,2}; & \ldots & A_{2,n}; & 1 \\
\cdot & \cdot & \cdot & \cdot & \cdot \\
A_{n,1}; & A_{n,2}; & \ldots & A_{n,n}; & 1 \\
1; & 1; & \ldots & 1; & 0
\end{array}
$$

where in general

$$
A_{r,s} = a_{r,s} + h_r + k_s,
$$

$h_1, h_2, \ldots h_n$ and $k_1, k_2, \ldots k_n$ being any two perfectly arbitrary

series of quantities. This simple transformation is of course derived by adding to the respective columns in the first matrix the last column (consisting of units) multiplied respectively by $h_1, h_2, \ldots h_n,$ 0; and to the respective lines, the last line (consisting of units) multiplied respectively by $k_1, k_2, \ldots k_n,$ 0.

Suppose, now, that we have two tetrahedrons whose volumes are represented respectively by one-sixth of the respective determinants

$$
\begin{array}{cccc}
x_1 & y_1 & z_1 & 1 \\
x_2 & y_2 & z_2 & 1 \\
x_3 & y_3 & z_3 & 1 \\
x_4 & y_4 & z_4 & 1
\end{array}
\qquad
\begin{array}{cccc}
\xi_1 & \eta_1 & \zeta_1 & 1 \\
\xi_2 & \eta_2 & \zeta_2 & 1 \\
\xi_3 & \eta_3 & \zeta_3 & 1 \\
\xi_4 & \eta_4 & \zeta_4 & 1
\end{array}
$$

x_r, y_r, z_r representing the orthogonal coordinates of the point r in one tetrahedron, and ξ_r, η_r, ζ_r the same for any point (r) in the other.

By the first theorem their product may be represented (striking off the last column only from each matrix) by the matrix

$$
\begin{array}{ccccc}
\Sigma x_1\xi_1; & \Sigma x_1\xi_2; & \Sigma x_1\xi_3; & \Sigma x_1\xi_4; & 1 \\
\Sigma x_2\xi_1; & \Sigma x_2\xi_2; & \Sigma x_2\xi_3; & \Sigma x_2\xi_4; & 1 \\
\Sigma x_3\xi_1; & \Sigma x_3\xi_2; & \Sigma x_3\xi_3; & \Sigma x_3\xi_4; & 1 \\
\Sigma x_4\xi_1; & \Sigma x_4\xi_2; & \Sigma x_4\xi_3; & \Sigma x_4\xi_4; & 1 \\
1; & 1; & 1; & 1; & 0
\end{array}
$$

where, in general, any such term as $\Sigma x_r . \xi_s$ represents

$$
x_r . \xi_s + y_r . \eta_s + z_r . \zeta_s.
$$

Again, by virtue of the second theorem, adding

$$
-\frac{1}{2}\Sigma x_1{}^2; \quad -\frac{1}{2}\Sigma x_2{}^2; \quad -\frac{1}{2}\Sigma x_3{}^2; \quad -\frac{1}{2}\Sigma x_4{}^2
$$

to the respective lines, and

$$
-\frac{1}{2}\Sigma\xi_1{}^2; \quad -\frac{1}{2}\Sigma\xi_2{}^2; \quad -\frac{1}{2}\Sigma\xi_3{}^2; \quad -\frac{1}{2}\Sigma\xi_4{}^2
$$

to the respective columns, the above matrix becomes (after a change of signs not affecting the result) the $-\frac{1}{6}$th of

$$
\left\{
\begin{array}{ccccc}
\Sigma(x_1-\xi_1)^2; & \Sigma(x_1-\xi_2)^2; & \Sigma(x_1-\xi_3)^2; & \Sigma(x_1-\xi_4)^2; & 1 \\
\Sigma(x_2-\xi_1)^2; & \Sigma(x_2-\xi_2)^2; & \Sigma(x_2-\xi_3)^2; & \Sigma(x_2-\xi_4)^2; & 1 \\
\Sigma(x_3-\xi_1)^2; & \Sigma(x_3-\xi_2)^2; & \Sigma(x_3-\xi_3)^2; & \Sigma(x_3-\xi_4)^2; & 1 \\
\Sigma(x_4-\xi_1)^2; & \Sigma(x_4-\xi_2)^2; & \Sigma(x_4-\xi_3)^2; & \Sigma(x_4-\xi_4)^2; & 1 \\
1; & 1; & 1; & 1; & 0
\end{array}
\right\}
$$

or calling the angular points of the one tetrahedron $a, b, c, d,$

Z 2

and of the other p, q, r, s, 8×36, *i. e.* 288 times, their product is representable by $-1 \times$ the determinant

$$\begin{vmatrix} (ap)^2; & (aq)^2; & (ar)^2; & (as)^2; & 1 \\ (bp)^2; & (bq)^2; & (br)^2; & (bs)^2; & 1 \\ (cp)^2; & (cq)^2; & (cr)^2; & (cs)^2; & 1 \\ (dp)^2; & (dq)^2; & (dr)^2; & (ds)^2; & 1 \\ 1; & 1; & 1; & 1; & 0 \end{vmatrix}$$

and of course if p, q, r, s coincide respectively with a, b, c, d, 576 times the square of the tetrahedron *abcd* will be represented under Mr. Cayley's form,

$$\begin{vmatrix} 0; & (ab)^2; & (ac)^2; & (ad)^2; & 1 \\ (ba)^2; & 0; & (bc)^2; & (bd)^2; & 1 \\ (ca)^2; & (cb)^2; & 0; & (cd)^2; & 1\,* \\ (da)^2; & (db)^2; & (dc)^2; & 0; & 1 \\ 1; & 1; & 1; & 1; & 0 \end{vmatrix}$$

four out of the sixteen distances vanishing, and the remaining twelve reducing to six pairs of equal distances. The demonstration of Staudt's theorem for triangles is obtained in precisely the same way by throwing the product of the two determinants

$$\begin{vmatrix} x_1 & y_1 & 1 \\ x_2 & y_2 & 1 \\ x_3 & y_3 & 1 \end{vmatrix} \quad \text{and} \quad \begin{vmatrix} \xi_1 & \eta_1 & 1 \\ \xi_2 & \eta_2 & 1 \\ \xi_3 & \eta_3 & 1 \end{vmatrix}$$

under the form of $-\frac{1}{4}$th of

$$\begin{vmatrix} \Sigma(x_1-\xi_1)^2; & \Sigma(x_1-\xi_2)^2; & \Sigma(x_1-\xi_3)^2; & 1 \\ \Sigma(x_2-\xi_1)^2; & \Sigma(x_2-\xi_2)^2; & \Sigma(x_2-\xi_3)^2; & 1 \\ \Sigma(x_3-\xi_1)^2; & \Sigma(x_3-\xi_2)^2; & \Sigma(x_3-\xi_3)^2; & 1 \\ 1; & 1; & 1; & 0 \end{vmatrix}$$

When the two triangles coincide, calling their angular points a, b, c, the above written determinant becomes

$$\begin{vmatrix} 0; & (ab)^2; & (ac)^2; & 1 \\ (ba)^2; & 0; & (bc)^2; & 1 \\ (ca)^2; & (cb)^2; & 0; & 1 \\ 1; & 1; & 1; & \end{vmatrix}$$

* The corresponding quantity to the above determinant for the case of the triangle (hereafter given) is identical with the Norm to the sum of the sides. I have succeeded in finding the Factor (of ten dimensions in respect of the edges), which, multiplied by the above Determinant itself, expresses the Norm to the sum of the Faces, *i. e.* the superficial area of the Tetrahedron.

or

$$(ab)^4 + (ac)^4 + (bc)^4 - 2(ab)^2 \cdot (ac)^2 - 2(ab)^2(bc)^2 - 2(ac)^2 \cdot (bc)^2 ;$$

the negative of which is the well-known form expressing the square of four times the area of the triangle *abc*.

There is another and more general theorem of Staudt for two triangles not in the same plane, which may be obtained with equal facility. In fact, if we start from the determinant

$$\begin{vmatrix} (a\alpha)^2 & (a\beta)^2 & (a\gamma)^2 & 1 \\ (b\alpha)^2 & (b\beta)^2 & (b\gamma^2) & 1 \\ (c\alpha)^2 & (c\beta)^2 & (c\gamma)^2 & 1 \\ 1 & 1 & 1 & \end{vmatrix}$$

and add to each column respectively the last column multiplied by $e\xi_1^2$, $e\xi_2^2$, $e\xi_3^2$ respectively, we arrive at the form

$$\begin{vmatrix} (a\alpha)^2 + e\xi_1^2 & (a\beta)^2 + e\xi_2^2 & (a\gamma)^2 + e\xi_3^2 & 1 \\ (b\alpha)^2 + e\xi_1^2 & (b\beta)^2 + e\xi_2^2 & (b\gamma)^2 + e\xi_3^2 & 1 \\ (c\alpha)^2 + e\xi_1^2 & (c\beta)^2 + e\xi_2^2 & (c\gamma)^2 + e\xi_3^2 & 1 \\ 1 & 1 & 1 & \end{vmatrix}$$

And considering ξ_1, η_1; ξ_2, η_2; ξ_3, η_3 as the coordinates of α, β, γ, the projections upon the plane of *abc* of a triangle ABC, whose plane intersects the former plane in the axis of y, and makes with that plane an angle whose tangent is (e), it is easily seen that this determinant is term for term identical with the determinant

$$\begin{vmatrix} (a\text{A})^2 ; & (a\text{B})^2 ; & (a\text{C})^2 ; & 1 \\ (b\text{A})^2 ; & (b\text{B})^2 ; & (b\text{C})^2 ; & 1 \\ (c\text{A})^2 ; & (c\text{B})^2 ; & (c\text{C})^2 ; & 1 \\ 1 ; & 1 ; & 1 ; & 0 \end{vmatrix}$$

which therefore expresses -16 times the product of the triangles *abc* and *αβγ*, *i. e.* *abc* × ABC × cosine of the angle between the two. A similar method, if we ascend from sensible to rational geometry, may be given for expressing in terms of the distances the product of *any* two pyramids (in a hyperspace) by the cosine of the angle included between the two infinite spaces* in which they respectively lie. To pass from the cases which have been considered of two triangles to two polygons, or of two tetrahedrons to two polyhedrons, generally presents no difficulty ; and for Professor Staudt's

---

* In rational or universal geometry, that which is commonly termed infinite space (as if it were something absolute and unique, and to which, by the conditions of our being, the representative power of the understanding is limited), is regarded as a single homaloid related to a plane, precisely in the same way as a plane is to a right line. Universal geometry brings home to the mind with an irresistible force of conviction the truth of the Kantian doctrine of locality.

method of doing so, which is simple and ingenious, and does not admit of material improvement, the reader is referred to the memoir in Crelle's Journal or Terquem's *Annales* already adverted to. It is, however, to be remarked (and this does not appear to be sufficiently noticed in the memoirs referred to), that whilst the expression for the product of any two polygons in terms of the distances given by Staudt's theorem is unique, that for the product of two polyhedrons given by the same is not so, but will admit of as many varieties of representation as there are units in the product of the numbers respectively expressing the number of ways in which each polygonal face of each polyhedron admits of being mapped out into triangles. I cannot help conjecturing (and it is to be wished that Professor Staudt or some other geometrician would consider this point) that in every case there exists, linearly derivable from Staudt's optional formulæ (but not coincident with any one of them), some unique and best, because most symmetrical, formula for expressing the product of two polyhedrons in terms of the distances of the angular points of the one from those of the other. In conclusion I may observe, that there is a theorem for distances measured on a given straight line, which, although not mentioned by Staudt, belongs to precisely the same class as his theorems for areas in a plane and volumes in space; viz. a theorem which expresses twice the rectangle of any two such distances under the form of an aggregate of four squares, two taken positively and two negatively; that is to say, if A, B, C, D be any four points on a right line $2AB \times CD = AD^2 + BC^2 - AC^2 - BD^2$. I know not whether this theorem be new, but it is one which evidently must be of considerable utility to the practical geometer.

Note on the above.

The fundamental theorem in determinants, published by me in the Philosophical Magazine in the course of last year, leads immediately to a class of theorems strongly resembling, and doubtless intimately connected with, those of Staudt.

Thus for triangles we have by this fundamental theorem

$$
\begin{vmatrix} x_1 & x_2 & x_3 \\ y_1 & y_2 & y_3 \\ 1 & 1 & 1 \end{vmatrix} \times \begin{vmatrix} \xi_1 & \xi_2 & \xi_3 \\ \eta_1 & \eta_2 & \eta_3 \\ 1 & 1 & 1 \end{vmatrix}
$$

$$
= \begin{vmatrix} x_1 & \xi_1 & \xi_2 \\ y_1 & \eta_1 & \eta_2 \\ 1 & 1 & 1 \end{vmatrix} \times \begin{vmatrix} \xi_3 & x_2 & x_3 \\ \eta_3 & y_2 & y_3 \\ 1 & 1 & 1 \end{vmatrix} + \begin{vmatrix} x_1 & \xi_2 & \xi_3 \\ y_1 & \eta_2 & \eta_3 \\ 1 & 1 & 1 \end{vmatrix} \times \begin{vmatrix} \xi_1 & x_2 & x_3 \\ \eta_1 & y_2 & y_3 \\ 1 & 1 & 1 \end{vmatrix}
$$

$$
+ \begin{vmatrix} x_1 & \xi_3 & \xi_1 \\ y_1 & \eta_3 & \eta_1 \\ 1 & 1 & 1 \end{vmatrix} \times \begin{vmatrix} \xi_2 & x_2 & x_3 \\ \eta_2 & y_2 & y_3 \\ 1 & 1 & 1 \end{vmatrix}
$$

and consequently, if ABC, DEF be any two triangles,

$$ABC \times DEF = ADE \times FBC + AEF \times DBC + AFD \times BCE.$$

This may be considered a theorem relating to two ternary systems of points in a plane. The analogous and similarly obtainable theorem for two binary systems of points in the same right line is $AB \times CD = AC \times DB - AD \times CB$. As in applying this last theorem to obtain correct numerical results we must give the same algebraical sign to any two lengths denoted by the two *arrangements* XY, ZT, according as the direction from X to Y is the same as that from Z to T, or contrary to it, so in the theorem for the products of triangles, the areas denoted by any two ternary arrangements XYZ, TUV must be taken with the like or the contrary sign, according as the direction of the rotation XYZ is consentient with or contrary to that of TUV; so that three of the six possible arrangements of XYZ may be used indifferently for one another, but the other three would imply a change of sign. If we analyse what we mean by fixing the direction of the rotation of XYZ, and reduce this form of speech to its simplest terms, we easily see that it amounts to ascertaining on which side of B, C lies, *i. e.* whether to its right or left, to a spectator stationed at A on a given side of the plane ABC.

Let us now pass to the corresponding theorems for two tetrahedrons put respectively under the forms

$$
\begin{array}{cccc}
x_1 & x_2 & x_3 & x_4 \\
y_1 & y_2 & y_3 & y_4 \\
z_1 & z_2 & z_3 & z_4 \\
1 & 1 & 1 & 1
\end{array}
\qquad
\begin{array}{cccc}
\xi_1 & \xi_2 & \xi_3 & \xi_4 \\
\eta_1 & \eta_2 & \eta_3 & \eta_4 \\
\zeta_1 & \zeta_2 & \zeta_3 & \zeta_4 \\
1 & 1 & 1 & 1
\end{array}
$$

We may represent this product in either of two ways by the application of our fundamental theorem, viz. as

$$
\begin{array}{cccc}
x_1 & \xi_1 & \xi_2 & \xi_3 \\
y_1 & \eta_1 & \eta_2 & \eta_3 \\
z_1 & \zeta_1 & \zeta_2 & \zeta_3 \\
1 & 1 & 1 & 1
\end{array}
\times
\begin{array}{cccc}
\xi_4 & x_2 & x_3 & x_4 \\
\eta_4 & y_2 & y_3 & y_4 \\
\zeta_4 & z_2 & z_3 & z_4 \\
1 & 1 & 1 & 1
\end{array}
+ \&c.
$$

or as

$$
\begin{array}{cccc}
x_1 & x_2 & \xi_1 & \xi_2 \\
y_1 & y_2 & \eta_1 & \eta_2 \\
z_1 & z_2 & \zeta_1 & \zeta_2 \\
1 & 1 & 1 & 1
\end{array}
\times
\begin{array}{cccc}
\xi_3 & \xi_4 & x_3 & x_4 \\
\eta_3 & \eta_4 & y_3 & y_4 \\
\zeta_3 & \zeta_4 & z_3 & z_4 \\
1 & 1 & 1 & 1
\end{array}
+ \&c.
$$

there being four products to be added together in the first expression and six in the latter; and the rule, if we wish that all the products may be additive, being that on removing the sign of multiplication the determinant to the square matrix formed

by the Greek letters *in situ* shall always preserve the same sign. Hence we derive two geometrical formulæ concerning the products of polyhedrons, viz.

(1.) $ABCD \times EFGH = ABCE \times FGHD - ABCF \times GHED$
$+ ABCG \times HEFD - ABCH \times FGED.$

(2.) $ABCD \times EFGH = ABEF \times GHCD + ABGH \times EFCD$
$+ ABEG \times HFCD + ABHF \times EGCD$
$+ ABEH \times FGCD + ABFG \times EHCD.$

These formulæ give rise to an exceedingly interesting observation. In order that they shall be numerically true, we must have a rule for fixing the sign to be given to the solid content represented by any reading off of the four points of a tetrahedron, *i. e.* we must have a rule for determining the sign of solid contents of figures situated anywhere in space analogous to that which, as applied to linear distances reckoned on a given right line, is the true foundation of the language of trigonometry, and the condition precedent for the possibility of any system of analytical geometry such as exists, and which, not altogether without surprise, I have observed in the pages of this Magazine one of the learned contributors has thought it necessary to vindicate the propriety of importing into his theory of quaternions.

Various rules may be given for fixing the sign of a tetrahedron denoted by a given order of four letters. One is the following: the content of ABCD is to be taken positive or negative, according as to a spectator at A the rotation of BCD is positive or negative. Another, again, is to consider AB and CD as representing, say two electrical currents, and to suppose a spectator so placed that the current AB shall pass through the longitudinal axis of his body from the head towards the feet, and looking towards the other current CD; the sign of the solid content of the tetrahedron (and, indeed, also the effect, in a general sense, of the action of the two currents upon one another) will depend upon the circumstance of this latter current appearing to flow from the right to the left, or contrariwise in respect of the spectator. Last and simplest mode of all, the sign of the solid content of ABCD will depend upon the *nature* (in respect to its being a right-handed or left-handed-screw) of any regular screwline (whether the common helix or one in which the increase or decrease of the inclination is always in the same direction) terminating at B and C, and so taken that BA shall be the direction of the tangent produced at B, and CD the direction of the tangent produced at C. Inasmuch as of the twenty-four permutations of a quaternary arrangement a defined twelve have one sign, and the other twelve the contrary sign, these various de-

finitions of the direction, or, as it may be termed, polarity, of a tetrahedron corresponding to a given reading, whether as taken each in itself or compared one with another, give rise to, or rather imply a considerable number of interesting theorems included in our intuitions of space, and probably belonging to the, in my belief, inexhaustible class of primary and indemonstrable truths of the understanding.

7 New Square, Lincoln's Inn,
    October 2, 1852.

LIV. *On Copper Smelting.* By JAMES NAPIER, *F.C.S.**

[Continued from p. 271.]

Construction of the Furnaces, &c.

IN my last communication I mentioned that the kind or quality of the fuel has often to be regulated by the nature of the materials forming the furnace; for while it is the object of the smelter to get his charge fused in the shortest possible time, it is also necessary to prevent fusing the bricks and other materials composing the furnace. It is therefore a matter of great consequence that the best materials be used in its construction: they should not be easily acted upon by heat, nor by the matters fused upon or in contact with them; such materials not only last longer, but they allow the smelting operations to be done with greater facility and perfection.

It may be necessary, in the first place, to endeavour to describe the nature of the common furnaces in use, reserving any remarks upon peculiarity of construction for particular operations until describing these operations.

Calcining-furnace.—The object of this furnace is to keep the ore, or whatever matters are operated upon, exposed to a red heat in a free current of air, for the purpose of burning off or volatilizing the sulphur, arsenic, antimony, or other volatile matters, and oxidizing the iron. This furnace is never used for fusion, and the materials composing it are not so carefully selected or so refractory as for a fusing-furnace. The mode of construction is generally adapted to the object of it, and whether for ore or *metal*. The common *calciner* ranges from 18 to 22 feet in length, and from 11 to 14 feet in breadth outside measure, independent of fire-place; the roof of the chamber internally above the floor of the hearth is from 2 to 3 feet. The whole furnace is built upon arches, the vaults of which serve to receive the ore from the fur-

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nace, when calcined, by means of holes in the floor of the furnace or hearth through the roof of the arches.

Each calcining-furnace has generally a shaft or chimney of its own about 40 feet high, except where there is one general shaft. When the fumes from the calciner are led through a culvert under ground to the main shaft, which, as will be shown in the sequel, is the best and most œconomical, much stuff is caught in these culverts that is useful.

It need hardly be mentioned, that these furnaces have to be firmly tied or bound together by means of iron, having large upright cast iron studs opposite each other round the furnace, and these held together by means of iron rods passing over and through the building, which the men have to watch and tighten up or ease as the expansion or contraction requires; otherwise the furnace is liable to crack, or, as sometimes takes place, the bindings break and the furnace splits in two. The furnace is lined inside with fire-brick built in with fire-clay, so is the fire-place and bridge. The shaft or chimney has also to be lined with fire- or flintshire-brick. The cost of an average-sized calcining-furnace without shaft is about £150.

A *Fusing-furnace* is constructed differently from the calciner, the object being to get a high heat concentrated in the hearth. The ordinary size of the hearth of a fusing-furnace is 13 feet long by 8 wide inside measure, of an oval form, resembling the section of an egg. Generally these furnaces have each a stack or chimney of its own. The inside of the hearth and fireplace is lined with the best fire-brick; so is the inside of the lower part of the chimney, passing into flintshire-brick as it ascends. These are also bound about with iron studs and bars, and some are cased all round with cast iron. A furnace of the size above named with stack attached will cost about £130 before anything be fused in it.

The *Roasting-furnace* differs little from the fusing-furnace. They are generally larger in size, and have an opening or door in the side of the hearth for the purpose of charging, as the matters are put into it in large pieces which could not pass through a hopper. This door is also occasionally opened during the roasting to admit air to the fused materials. The roasting-furnace is also furnished with air-holes through the bridge, similar to what is used in the calciner; the materials used in the construction of these furnaces must also be of the best quality.

The *Refining-furnace* is similar to the roasting-furnace, but is generally a little more oval; it has also a side door for the introduction of the metal, and there is a well left in the bottom of the hearth close to the front door to allow the metal to be ladled out to the last portions.

These brief descriptions will enable us now to consider the nature of the materials best fitted for forming the different parts of the furnace; and although modifications and improvements have been proposed in the construction of some of these furnaces, which will be referred to afterwards, they do not affect the present inquiry.

The hearth or chamber of the furnace may be looked upon as a large crucible, in which the ore or other matters undergoing the process of manufacture are melted; it will therefore be obvious that the matters forming this crucible must be capable of withstanding a higher heat before melting than the substances put into it. Lime, silica, alumina, &c. *alone*, will stand any degree of heat before fusing; but a mixture of these at a high heat would combine and form a fusible substance; hence the relation of the materials forming the lining of the furnace with the matters to be heated in it has also to be considered. Thus, were we to line a furnace with pure silica, which no furnace heat would touch, and heat in that furnace lime, oxide of iron, oxide of copper, &c., the silica and these oxides would soon fuse and form glass; so that great care is required to prevent this combination of the substances with the bricks undergoing fusion. Thus some kinds of brick will answer for one sort of furnace and not for another; one kind may stand well in the fire-place, and not be suited for the hearth, and *vice versâ.* Some kinds will stand in the melted matter that will not stand on the roof exposed to the fumes; bricks with much alumina in them will not stand exposed to melted copper. All kinds of brick or clay give way rapidly round the sides of the furnace at a line corresponding to the surface of the fused metal. It need hardly be stated that these remarks are equally applicable to the clay or other matters used for making the bricks; for good bricks are of little avail if there be not equally good clay to bind them together.

Another property requisite in the bricks and clay used for lining furnaces is, that they must not be liable to crack, either by the intense heat or by a sudden current of cold air passing over them when hot, which in furnaces cannot be altogether avoided. A crack taking place in the bottom or sides of a furnace is a most serious affair; neither should the bricks be porous. In making crucibles, their liability to crack is lessened by mixing with the clay some sand, ground fire-brick, and graphite. Hessian crucibles, the best of all clay crucibles, are made by mixing the clay with half its weight of sand. Black-lead crucibles are made by mixing graphite or plumbago with the clay; but when too much of these materials is used, the crucible becomes porous, and allows the matters fused in them to pass through. The fol-

lowing table from Berthier of the composition of crucibles and pots known to stand well in working, will serve as data for comparing with the bricks and bottoms of the copper furnaces.

	Silica.	Alumina.	Oxide of iron.	Magnesia.
Hessian crucibles	71	25	4	
Paris (Beaufoy's)	65	34	10	
Saveignies, near Beauvois...	72	19	4	
English, for casting steel ...	71	23	4	
St. Etienne, for casting steel	65	25	7	
Glass pots, Nemours	67	32	1	
Glass pots, Bohemia	68	29	2	Trace.

We may here remark generally, that the bricks which contain the most silica stand the action both of fire and melting matters best.

Flintshire brick.—This sort is much used in the construction of furnaces and chimneys in the copper-works of Wales; they are not used for lining the furnaces where the melted matters are to come into contact with them, but in parts exposed to great heat and air-currents. Their analysis gave—

$$
\begin{array}{lr}
\text{Silica} & 88.1 \\
\text{Alumina} & 4.5 \\
\text{Protoxide of iron} & 6.1 \\
\text{Lime} & 1.2 \\
\hline
& 99.9
\end{array}
$$

These bricks are of a blackish-red colour and very hard.

Fire-bricks from Lysnewydd, South Wales.—Used in large quantity in the copper-works, both for fire-places and hearths.

$$
\begin{array}{lr}
\text{Silica} & 84.0 \\
\text{Alumina} & 14.1 \\
\text{Lime} & .7 \\
\text{Protoxide of iron} & .5 \\
\hline
& 99.3
\end{array}
$$

Dinnas bricks, sometimes termed stone bricks.—These may be considered indispensable in the process of copper smelting. They are used where melted copper has to come into contact with the furnace, but are very soon corroded when exposed to the influence of oxide of copper. They are a coarse-grained brick, resembling in appearance a conglomeration of small pieces of quartz rather than an artificial brick. Their composition is—

	From Penderyn.		From Dinnas.	
	No. 1.	No. 2.	No. 1.	No. 2.
Silica	95·53	94·05	100	91·95
Alumina	2·67	4·55	trace	8·05
Protoxide of iron .	·44	...	trace	
Lime	·82			
	99·46	98·60		100·00

The first analysis given is of a large piece of brick ground up, and which may be taken as a sample of the mass; but the quantity of lime and clay is not sufficient to bind the whole silica, were they intimately mixed; but the silica being in pieces about the size of an ordinary pea, the alumina and lime causing their adhesion is in proper proportion to form clay, and is very plastic. If only a small portion of such bricks be taken for analysis, nearly pure silica may be obtained, as shown in the third column of the above table. The matters composing these bricks are obtained from a quarry in the neighbourhood of Neath. These matters are crushed under a stone, the materials are then wetted and the mould for the brick filled. As the plastic ingredients in these bricks are mechanically mixed, not in chemical union with the quartz, we believe that ground or crushed quartz mixed with a small portion of alumina and lime, as shown in the above analysis, or good fire-clay, would serve the same purpose. Indeed in Chili such has been tried with success, and it might be much more extensively adopted. These bricks expand by heat more than other fire-bricks; but they do not contract to the same extent, which is a valuable property in reference to maintaining a solid and close lining.

Some Dinnas brick from the roof of a refining furnace much exposed to the escape of oxide of copper, and also to metallic copper being spurted upon them during poling, and which was consequently corroded and left spongy, gave by analysis—

Silica	72·2
Oxide of copper . .	27·0
Oxide of iron . . .	·6
	99·8

Another quality of fire-brick sometimes used in the copper works is from Pembroke.

The following analysis of these is by Mr. John Cameron, of Spitty Works, to whom we are indebted for several analyses in these papers.

Silica	88·43
Alumina	6·90
Oxide of iron . .	1·50
Lime	3·40
Magnesia	trace
	100·23

The best Newcastle and Stourbridge bricks are equal in quality for any of the purposes for which the above or Lysnewydd are used, and often preferable, but they are not so generally employed. However, none of the ordinary fire-bricks can replace the Dinnas in the uses to which they are applied.

The fire-clay used for making these bricks should possess similar properties to the brick. It may be considered the same materials unaltered by fire; and the same rule applies, viz. the more silica they contain consistent with their solidity and binding qualities, the better they answer the purposes of standing high temperatures. Dinnas clay, that used for binding Dinnas brick, is simply the materials of which the bricks are formed, made moist by water.

The following table of analyses of fire-clays from three localities far apart, will serve as an illustration of *quality*.

	Stourbridge.		Monmouth *.		Govan †.	
Silica	70·4	63·5	75·3	80·1	60·2	59·7
Alumina.........	22·7	22·0	16·8	17·9	37·7	37·5
Oxide of iron ...	2	2·9	1·0	1·0	1·0	2·3
Lime	·5	·8	·9	1·0	1·0	1·0
Water............	4·4	10·8	6			
Magnesia	trace	trace				
	100·0	100·0	100·0	100·0	100·0	100·0

Such, then, is the general chemical character of the fire-brick and fire-clay used in lining the fusing-furnaces in copper smelting. Chemical investigation and practical experience have not yet been sufficiently and simultaneously carried on to enable analysis to supersede an actual trial; nevertheless it is a very good guide; and that it is not now more certain, is owing rather to the paucity of such investigations with a due observation of facts in relation to analyses than to any discrepancy between principle and practice.

These fire-bricks and clay are only used to line the side walls and form the roof of the furnace, but they are not for the bottom of hearths. When bricks, however refractory, are used for a bottom, the melted stuff finds its way under and raises them up. The bottom of a fusing-furnace, however large, must be one solid piece, and this is obtained by using sand. A fusing-furnace, as it is now built, stands upon an arch running the whole length and breadth of the furnace, and brought up square; upon this rise the side walls, which we have been describing as being lined with fire-bricks; the intervening space is where the sand bottom is laid, which averages from 18 inches to 2 feet in depth. When

* Mr. C. Cowper.　　　　† Dr. Penny.

a furnace is built, a fire is kept in it until it is considered to be sufficiently dry and annealed to be fit for working. The bottom is made as follows :—Sand is laid upon the brick bottom to the depth of about 18 inches, which is kept at a red heat for about 24 hours, when a little slag is laid on the top of the sand ; the furnace doors are all closed, and the heat increased to the melting-point : the slag fuses over the surface, penetrating into and combining with the sand to about the depth of an inch ; when this is accomplished, the heat of the furnace is gradually lowered. The whole surface of the sand bottom has now become one piece of hard glass, adhering firmly to the side walls, forming a complete shallow vessel. This forms what is termed the *true* or *lower* bottom of the furnace. Upon the top of this bottom is put a second layer of sand about 6 inches deep, which is treated in the same manner, and is termed the false or upper bottom. This upper bottom is of great practical value, as the bottom, exposed to cold currents, is always liable to crack ; or it may be separated slightly from the side walls, and allow the fluid to pass through and under; but it only passes to the surface of the lower bottom, which is not affected by the crack, flows over it, and raises the upper in pieces, which is easily withdrawn, and a new upper bottom put in. Were it not for this provision, and an opening or crack taking place in the main bottom, or a piece of it breaking and coming up, the renewing of a bottom would be a most serious matter, and the copper constantly penetrating would be a source of great loss. As it is, there is a constant penetration of the melted matter, more or less according to the nature of the sand, the care of the workman, and the condition of the copper in process of manufacture. The nearer it approaches the condition of metal, the greater the liability to penetrate. Some bottoms of refining and roasting-furnaces have been often known to contain from twenty to thirty tons of copper ; indeed, so great is the amount of capital absorbed in this way, that the average value of each furnace used for fusing may be reckoned at £500 after one year's working.

Formerly, instead of the furnace being built upon an arch, as above described, it was one solid mass of brickwork, in which case the penetration of metal was much greater, as the free current of air passing through the arch keeps the bottom cool and prevents the metal from running through.

A plan for lessening the quantity of copper penetrating into the bottom was proposed a few years ago, and we believe is the subject of a patent. Instead of an arch of bricks, the side walls are started from the ground, and the sand bottom is supported by iron plates laid across and resting upon the side walls at the proper height. Trials with these iron bottom supports did lessen the penetration a little, but the great liability of the iron plate

to burn through rendered them useless. We have seen one in a refining furnace give way in a few weeks, and a charge of seven tons of copper run into the ash-pit, leaving no remedy but to pull down the furnace to the ground.

We have tried bottoms constructed with bricks and tiles made of clay and plumbago, and these grooved and tongued to fit into each other, as the deals of a floor, and cemented together by the same materials as they were made of. Such bottoms were perfectly impervious to copper; but they were liable, with the slightest inattention, to open at the joints and allow the copper to pass under and raise the whole. We have also laid the whole bottom with clay and graphite, baking it carefully to make one solid piece. Such bottoms we have had in work for five weeks without any copper penetrating; but although made about three inches thick, it was worn through in that time, and required as many days to renew as a sand bottom requires hours, and which if the sand be good will last longer. This loss of time is of so much importance where there are forty or fifty furnaces, as to leave no doubt which to adopt. When a sand bottom is to be renewed, or when it breaks, the fire is kept up for some time, then a hole is made in the side of the furnace under the top bottom, when the copper flows out, where the two bottoms join. The heat is continued until as much of the melted matters sweats out as possible, leaving the sand less tenacious; it is then drawn out by iron *rakes* or *rabbles*. The old bottoms are broken up and crushed fine, and put into the fusing-furnace with the ore. These old bottoms, with old lining bricks and clay, are termed *cobbing,* a term we may have occasion to use again.

From what has been said respecting the necessity of obtaining good bricks and clay, it will be obvious that the same care must apply to the selection of the sand for bottoms. If the sand be too coarse, the bottom will be porous; if too fine, there is great liability to crack and break up; should it contain matters that make it fusible, it soon melts away and mixes with the slags; the sand should therefore be as pure silica as possible. The proper physical qualities of sand best suited for bottoms are easily ascertained by a little experience and observation; but the chemical character, upon which a great deal depends, must be found by experiment. The following analyses of a few sands, either in use or tried for bottoms, will serve as data to judge of the qualities of a sand.

From *Coadyall,* Wales :—

Silica	92·0
Peroxide of iron	3·2
Alumina	2·8
Lime	1·3
	99·3

The average wear of bottoms made with this quality of sand was four weeks to five weeks, when they melted away or became thin.

Cwm Ivy, Wales :—

Silica	89·8
Peroxide of iron	4·4
Lime	2·8
Magnesia	1·6
Carbonaceous matters	·9
	99·5

The average wear of bottoms of this sand was two weeks.

Pembre, Wales :—

No. 1. (Mr. Cameron.)		No. 2. (Mr. Field.)	
Silica	93	Silica	92·20
Oxide of iron and alumina	4	Carb. of lime	3·46
		Magnesia	0·06
Lime	3	Alumina	1·56
	100	Oxide of iron	1·84
			100·12

Average wear of bottoms made of these sands was six weeks.

But the name of locality is no guarantee as to quality, as the following two analyses will show.

Pembre :—

	No. 1.	No. 2.
Silica	87·40	86·66
Oxide of iron	3·06	5·80
Lime	7·80	5·60
Magnesia and soda	1·74	1·94
	100·00	100·00

Bottoms made with these did not stand longer than two weeks, and proved liable to break up in a few days.

Bow Common, London, found a little under the surface and washed before using :—

Silica	96·5
Oxide of iron	3·3
Lime and magnesia	·2
	100·0

The average wear of bottoms made of this sand was between two and three months, showing how analysis and practice agree, and also that the best bottoms are made with sand containing most silica.

An aluminous or clay sand, although sufficiently refractory as

regards heat, does not make a good bottom for furnaces where
copper is to come into contact with it.   The following were found
quite unsuitable :—

		(Mr. Field.)
Silica	85·20	80·4
Alumina . . .	12·75	17·3
Lime	·60	2·4
Oxide of iron . .	·89	
Magnesia . . .	·20	
	99·64	100·0

Alumina seems to be the deleterious ingredient in this sand,
and confirms our remarks in reference to fire-bricks and clay.
And this is fully borne out where sand with alumina is found
uncombined and capable of being separated by washing, as in
some parts of Australia.

Sand as dug up, unsuitable for bottoms, not lasting more than
ten or twelve days :—

	(A. D. Thomas.)
Silica	85·18
Alumina . . .	9·10
Oxide of iron . .	3·00
Lime	1·12
Magnesia . . .	1·60
	100·00

This being washed in a small running stream, gave a sand which
lasted as bottoms from six to eight weeks.   Analysis gave—

Silica	92·0
Carbonate of lime . . .	5·6
Carbonate of magnesia .	1·6
Oxide of iron and alumina	·8
	100·0

It is worthy of remark how a slight difference in the per-centage
of silica will effect the fusibility—the tear and wear of a bottom.

We are not aware that the Isle of Wight or American sands
used in making crystal have been tried for bottoms.   As they
contain upwards of $99\frac{1}{2}$ per cent. of silica, the bottoms would
probably prove very lasting ; but there might be some danger of
a liability to crack owing to the fineness of the grain.

Roofs of furnaces and lining of stacks or culverts near the fur-
nace are often affected by the volatilized matters passing over
them ; those from the calciner are not so destructive to the bricks,
the heat not being so intense as to cause combination.   The
volatile matters only condense and form a deposit or crust upon

the bricks, which is generally composed of sulphur, arsenic, antimony, iron and copper; but in the fusing-furnace the bricks are often consumed and worn quite thin. The destruction of the bricks is greatest when fluxes are used; and if these contain chlorides, soda or potash, the corrosion of the building extends a good way along the stack or culvert. Where copper is exposed in the furnace, the oxides burning off often combine with the bricks and fuse into a solid cake. These deposits and sublimations, their composition, and their causes, will be noticed in their proper place.

[To be continued.]

LV. *On the Causes of the Excess of the Mean Temperature of Rivers above that of the Atmosphere, recently observed by* M. Renou. *By* WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.S.E. &c.*[*]

IT appears from the *Comptes Rendus* for the 14th of June 1852, (vol. xxxiv. p. 916), that M. Renou of Vendôme has for four years made a series of daily observations on the temperature of the river Loir at that place, as compared with that of the atmosphere, and has found that the mean temperature of the river invariably exceeds that of the air.

His observations for 1851, being the only series yet published, show that this excess varied between $1\frac{1}{2}$ and 3 Centigrade degrees, and that its average amount was $2°·24$ Centigrade; the mean temperature of the river for the whole year having been $12°·08$ Cent., while that of the air was $9°·84$.

A similar result has been deduced from a smaller number of observations made on the Loire at Tours by M. Oscar Valin.

Those facts are interesting, not only in a purely meteorological point of view, but also as affording an illustration of an important principle in the theory of heat; and considering the ease with which observations similar to those of M. Renou may be made at any place where a meteorological register is kept in the neighbourhood of a river, they appear to be well worthy of the attention of those members of the British Association who make meteorology their study.

The object of this paper is to point out how observations on the excess of the mean temperature of rivers above that of the atmosphere may be made available for the advancement of our knowledge of the theory of heat.

As an argument favourable to the opinion suggested by

M. Renou himself and by M. Babinet, that the solar heat, absorbed and re-radiated by the bed of the river, is the principal cause of the elevation of its temperature, M. Renou cites the fact, that he has frequently observed a sudden elevation of temperature in the river immediately follow the appearance of the sun; but on the other hand it is to be remarked, that M. Renou has also observed great elevations of temperature take place in the water when the sun was not visible.

It is worthy of note, as tending to show that the solar radiation is not the principal cause of the excess of the temperature of rivers over that of the air, that according to the table of the monthly means of M. Renou's observations in 1851, this excess greatly exceeded its mean amount in November and December, months in which the solar radiation is weak; and that in December the monthly mean very nearly reached its maximum, having been $2°\cdot95$, while its actual maximum, in May, was $3°\cdot09$. It is also to be observed, that while the mean diurnal variation of temperature was $8°\cdot03$ for the air, it was only $0°\cdot65$ for the river.

When we consider that it has been proved experimentally by Mr. Joule, that the heat developed by the friction of all substances (including in that term the consumption of power by the agitation of fluids) bears a certain definite proportion to the mechanical power consumed, it appears probable that friction is an important cause of the elevation of the temperature of rivers above that of the contiguous air.

Let us suppose that a river flows in a uniform channel, having a uniform inclination, with a uniform velocity; and let

   i denote the rate of inclination of the channel;

   v the velocity of the current;

then iv represents the height through which each mass of water descends during unity of time, and also the mechanical power due to the descent of unity of weight of water during unity of time along the channel of the river in question

Now as the velocity of the current is uniform, this mechanical power must be entirely consumed by friction; that is to say, transformed into heat.   Let

K denote the dynamical specific heat of liquid water; that is to say, the height through which a given weight must descend in order to produce mechanical power sufficient to elevate the temperature of the same weight of water by one degree; then, according to Joule's experiments,

$$K = 1390 \text{ feet per Centigrade degree,}$$

and $$\frac{vi}{K}$$

represents the number of degrees of temperature generated in a mass of water by descending during unity of time along the channel of the river.

The temperature of the river will rise until the loss of heat by conduction, radiation and evaporation exactly balances the production of heat by friction. This loss of heat must be approximately proportional to the excess of the temperature of the water above that of the atmosphere.

Let C represent the loss of heat, in degrees, for one degree of excess of temperature, sustained by unity of weight of water through unity of surface exposed to the air;

C' the corresponding coefficient for the surface in contact with the bed of the channel.

Let M denote the volume of unity of weight of water, that is to say, 0·016 cubic foot per lb. avoirdupois.

Let s be the area of the transverse section of the river;

b the breadth of its surface;

p the periphery of its bed.

Then

$$\frac{Mb}{s}, \frac{Mp}{s}$$

are the areas exposed by unity of weight of water in the channel to the air and to the soil respectively; and, if

ΔT be the excess of the temperature of the river above that of the atmosphere,

$\Delta T'$ its excess above that of the soil,

the loss of heat by conduction, radiation and evaporation, in unity of time measured in degrees, will be represented by

$$\frac{M}{s}(Cb\Delta T + C'p\Delta T').$$

This quantity being made equal to the gain of heat by friction, we have for the condition of equilibrium of temperature the following equation :—

$$\frac{vi}{K} = \frac{M}{s}(Cb\Delta T + C'p\Delta T'). \quad \ldots \quad (1)$$

If the temperature of the air and of the soil be the same, so that $\Delta T = \Delta T'$, then this equation becomes

$$\left.\begin{array}{l} \dfrac{vi}{K} = \dfrac{M}{s}(Cb + C'p')\Delta T, \\[2mm] \text{or} \quad \Delta T = \dfrac{vis}{KM(Cb + C'p')}. \end{array}\right\} \quad \ldots \quad (2)$$

It thus appears, that by means of observations of the excess of the mean temperature of rivers above those of the atmosphere and of the soil, we may test the soundness of the supposition that that excess is wholly or partly produced by friction; and if that supposition be found to agree with the facts, we may calculate, from observations on different streams under different circumstances, the numerical values of the constants C and C′.

In order that the observations may be capable of yielding satisfactory results, they should be made upon a variety of streams of different forms of section, inclinations and velocities; and the part of each stream at which the temperatures are observed should have a form of section, an inclination, and a velocity, as nearly as possible uniform.

The following quantities should be observed:—

1. The inclination of the stream, i.
2. Its area of section, s.
3. The breadth of its surface, b.
4. The periphery of its bed, p.
5. The velocity of the current, v.
6. The mean temperature of the air.
7. The mean temperature of the soil of the bed.
8. The mean temperature of the stream.

The observations of temperature were made by M. Renou every day at the hours of 4 A.M., 6 A.M., and every hour till 10 P.M. inclusive.

The effect of the solar heat can easily be tested, either by observing the temperature of a piece of stagnant water adjoining a river, of equal depth and similar form and constitution of bed (for the temperature of such a piece of water would exhibit the effect of the solar radiation unaffected by that of friction), or by continuing the observations of the temperatures of the river and of the atmosphere during the night.

As I do not yet know the form, dimensions, inclination, or velocity of the Loir, I am for the present unable to illustrate the principles stated above by numerical examples.

LVI. *On the Reconcentration of the Mechanical Energy of the Universe.* By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.S.E. &c.*[*]

THE following remarks have been suggested by a paper by Professor William Thomson of Glasgow, on the tendency which exists in nature to the dissipation or indefinite diffusion of mechanical energy originally collected in stores of power.

The experimental evidence is every day accumulating, of a law which has long been conjectured to exist,—that all the different kinds of physical energy in the universe are mutually convertible, —that the total amount of physical energy, whether in the form of visible motion and mechanical power, or of heat, light, magnetism, electricity, or chemical agency, or in other forms not yet understood, is unchangeably the transformations of its different portions from one of those forms of power into another, and their transference from one portion of matter to another, constituting the phænomena which are the objects of experimental physics.

Professor William Thomson has pointed out the fact, that there exists (at least in the present state of the known world) a predominating tendency to the conversion of all the other forms of physical energy into heat, and to the uniform diffusion of all heat throughout all matter.   The form in which we generally find energy originally collected, is that of a store of chemical power, consisting of uncombined elements.   The combination of these elements produces energy in the form known by the name of electric currents, part only of which can be employed in analysing compounds, and thus reconverted into a store of chemical power; the remainder is necessarily converted into heat: a part only of this heat can be employed in analysing compounds, or in reproducing electric currents.   If the remainder of the heat be employed in expanding an elastic substance, it may be entirely converted into visible motion, or into a store of visible mechanical power (by raising weights, for example), provided the elastic substance is enabled to expand until its temperature falls to the point which corresponds to absolute privation of heat; but unless this condition be fulfilled, a certain proportion only of the heat, depending upon the range of temperature through which the elastic body works, can be converted, the rest remaining in the state of heat.   On the other hand, all visible motion is of necessity ultimately converted entirely into heat by the agency of friction.   There is thus, in the present state of the known world, a tendency towards the conversion of all physical energy into the sole form of heat.

Heat, moreover, tends to diffuse itself uniformly by conduction and radiation, until all matter shall have acquired the same temperature.

There is, consequently, Professor Thomson concludes, so far as we understand the present condition of the universe, a tendency towards a state in which all physical energy will be in the state of heat, and that heat so diffused that all matter will be at the same temperature; so that there will be an end of all physical phænomena.

Vast as this speculation may seem, it appears to be soundly

based on experimental data, and to represent truly the present condition of the universe, so far as we know it.

My object now is to point out how it is conceivable that, at some indefinitely distant period, an opposite condition of the world may take place, in which the energy which is now being diffused may be reconcentrated into foci, and stores of chemical power again produced from the inert compounds which are now being continually formed.

There must exist between the atmospheres of the heavenly bodies a material medium capable of transmitting light and heat; and it may be regarded as almost certain, that this interstellar medium is perfectly transparent and diathermanous; that is to say, that it is incapable of converting heat, or light (which is a species of heat), from the radiant into the fixed or conductible form.

If this be the case, the interstellar medium must be incapable of acquiring any temperature whatsoever; and all heat which arrives in the conductible form at the limits of the atmosphere of a star or planet, will there be totally converted, partly into ordinary motion, by the expansion of the atmosphere, and partly into the radiant form. The ordinary motion will again be converted into heat, so that *radiant heat* is the ultimate form to which all physical energy tends; and in this form it is, in the present condition of the world, diffusing itself from the heavenly bodies through the interstellar medium.

Let it now be supposed, that, in all directions round the visible world, the interstellar medium has bounds beyond which there is empty space.

If this conjecture be true, then on reaching those bounds the radiant heat of the world will be totally reflected, and will ultimately be reconcentrated into foci. At each of these foci the intensity of heat may be expected to be such, that should a star (being at that period an extinct mass of inert compounds) in the course of its motions arrive at that part of space, it will be vaporized and resolved into its elements; a store of chemical power being thus reproduced at the expense of a corresponding amount of radiant heat.

Thus it appears, that although, from what we can see of the known world, its condition seems to tend continually towards the equable diffusion, in the form of radiant heat, of all physical energy, the extinction of the stars, and the cessation of all phænomena, yet the world, as now created, may possibly be provided within itself with the means of reconcentrating its physical energies, and renewing its activity and life.

For aught we know, these opposite processes may go on together; and some of the luminous objects which we see in distant regions of space may be, not stars, but foci in the interstellar æther.

LVII. *Notice on Chloride of Arsenic.*
By Dr. Penny *and* William Wallace, *Esq.**

SOME time since we were led to undertake a series of expe-
riments on the properties and composition of the chloride
of arsenic, with the view partly of testing the accuracy of the
researches that have been made for determining the equivalent
of arsenic, and partly of ascertaining the availability of this sub-
stance for the separation of arsenic from other metals, as well as
from organic matters in toxicological inquiries. The publication
of some of our results has been anticipated by Dr. Fyfe's excel-
lent paper " On the Detection of Arsenic," published in the Phi-
losophical Magazine for December 1851. There are, however,
several points connected with the chemical history of this inter-
esting compound which have not yet been noticed, and we
therefore trust that a brief statement of our investigations may
not be altogether unacceptable to the Philosophical Society of
Glasgow.

Gmelin's Handbook of Chemistry contains a fair abstract of
the several researches that have been made upon the properties
of chloride of arsenic, with the results of its analysis by Dr. J.
Davy. It has been analysed more recently by Pelouze, who
employed it for the determination of the atomic weight of me-
tallic arsenic[†]. Its production in medico-legal investigations has
been incidentally noticed by several authorities; but its invari-
able formation by heat from arsenious acid in presence of hydro-
chloric acid has, we think, been frequently overlooked; and in
certain processes recommended for the separation of arsenic from
organic matters, its ready volatility would unquestionably be
very liable to occasion a loss of a portion of the metal.

To these points our attention has been particularly directed,
and our results fully confirm the several statements made by
Dr. Fyfe in the paper referred to. Before giving these results,
however, we shall describe the processes by which we obtained
anhydrous chloride in a state of purity, and the new method by
which we estimated the proportion of arsenic existing in it.

In one process for its preparation, powdered arsenious acid
was put into a retort with a considerable quantity of concen-
trated hydrochloric acid, and the mixture distilled. Anhydrous
chloride and a solution of chloride of arsenic in hydrochloric
acid soon collected in the receiver. The former was found at
the bottom as a dense oily liquid, and the other products floated
above. The anhydrous chloride may be easily separated with a

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* Communicated by the Authors, having been read at the Meeting of the
Philosophical Society of Glasgow, January 7, 1852.
† *Comptes Rendus*, vol. xx. p. 1047.

pipette, and purified by careful rectification. The principal objection to this process is, that when the hydrochloric acid is reduced below a certain point, the anhydrous chloride ceases to separate from the other distilled products, and thus a small proportion only is obtained. Our experiments lead us to infer that little, if any, anhydrous chloride can be obtained with acid below twenty per cent.

The second method consists in passing dry hydrochloric acid gas over powdered arsenious acid. The phænomena in this process are exceedingly interesting. The absorption of the gas is immediate, and a considerable elevation of temperature occurs, water and chloride of arsenic being simultaneously produced. Almost immediately on contact of the hydrochloric acid gas, the arsenious acid becomes moist and speedily disappears; and when the action is completed, two liquids are found in its place; the lower one being anhydrous chloride, and the upper one a saturated solution of hydrochloric acid in water, with a small quantity of dissolved chloride of arsenic. The non-production of hydrated chloride of arsenic in the above circumstances is somewhat remarkable, since the quantity of water produced by the double decomposition of the two acids is exactly the amount which is stated to exist in the hydrated chloride; thus

$$AsO^3 + 3HCl = AsCl^3 + 3HO.$$

In a particular experiment, we found that 50·9 grains of arsenious acid absorbed 63·5 grains of hydrochloric acid gas (dried by oil of vitriol and chloride of calcium), which corresponds very closely to one equivalent of anhydrous chloride, plus three equivalents of water saturated with hydrochloric acid. This is certainly the more elegant and more satisfactory process for the preparation of this substance. In both cases the resulting chloride is easily purified by distillation.

The methods for the quantitative estimation of arsenic are too well known to require notice. The new process which we employed for the analysis of the chloride, is based upon the reciprocal action of chromic and arsenious acids in presence of hydrochloric acid. The reaction is exhibited in the following equation :—

$$3AsCl^3 + 4CrO^3 + 3HO = 3AsO^5 + 2Cr^2 Cl^3 + 3HCl.$$
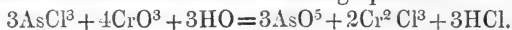
A weighed quantity of the pure chloride was mixed with water and caustic potash, and excess of hydrochloric acid subsequently added. The mixture being gently heated, a quantity of bichromate of potash, barely sufficient to peroxidize the arsenic, was slowly added. A weak solution of a known quantity of the same salt was then cautiously dropped into the liquid, till a portion taken out on the end of a rod gave a faint yellow tinge to a solution of acetate of lead spotted on a slab. The delicacy of

this test, as thus applied, for the detection of bichromate of potash has been fully shown in the fourth volume of the Quarterly Journal of the Chemical Society of London. Taking the mean of two well-executed experiments, it results that 100 parts of chloride of arsenic correspond to 54·5 parts of bichromate of potash. In order to deduce from this ratio the proportion of arsenic existing in the chloride, similar experiments were made with pure arsenious acid, which gave very nearly the proportion of 100 of arsenious acid to 100 of bichromate of potash. From these numbers it is evident that chloride of arsenic contains 41·25 per cent. of metallic arsenic.

The amount of chlorine was estimated in the usual manner as chloride of silver.

The following table shows the composition of the chloride:—

		Theory.	Experiment.
Arsenic	75	41·32	41·25
3 equivs. Chlorine .	106·5	58·68	58·86
	181·5	100·00	100·11

It may be as well to mention, that the chloride prepared as above had the specific gravity of 2·1766. It was strongly acid to litmus paper, completely soluble in alcohol and in æther; and it was also observed to have the power of dissolving a very considerable proportion of arsenious acid.

The action of water on the anhydrous chloride is particularly remarkable. It is stated by several writers, that the addition of the proper quantity of water converts the anhydrous chloride into a hydrate, having the formula $AsCl^3, 3HO$. Our attempts to obtain this hydrated compound proved unsuccessful. A known quantity of the anhydrous chloride was mixed with a sufficient proportion of water to convert it into the hydrate referred to, and the two liquids well agitated. The mixture became perceptibly warm, but on repose the greater part of the chloride separated. Additional portions of water were then successively added, the mixture being allowed to cool after each addition. The chloride gradually diminished; and when the total quantity of water amounted to about 18 equivalents to one of the chloride, the latter was found to be completely mixed. The resulting fluid, which had the specific gravity of 1·53, is miscible with a much larger quantity of water without any visible change, viz. to the extent of nearly 18 equivalents additional, making a total of 36 equivalents of water to one of anhydrous chloride. On the further addition of water, however, a separation of arsenious acid takes place. The specific gravity of the mixture containing 36 equivalents of water was 1·346.

We have obtained some interesting results by the distillation

of the two fluids, containing respectively 18 and 36 equivalents of water, an account of which we reserve for a future communication.

With respect to the heat evolved on mixing anhydrous chloride and water, we may state that in a particular experiment, in which 117 grains of the former were briskly agitated with a quantity of the latter corresponding to 18 equivalents, the temperature rose from 60° to 113° F.   Then on allowing this mixture to cool, and pouring in 18 additional equivalents of water, the temperature increased from 60° to 94°.

From our experiments on this part of the subject, it appears to us very questionable whether such a hydrate as $AsCl^3$, $3HO$ has been obtained.

Several experiments were made for the purpose of ascertaining the precise conditions necessary for the production and volatilization of the chloride from a heated mixture of arsenious and hydrochloric acids.   The most important result was, that chloride of arsenic may be detected in the distillate as soon as hydrochloric acid itself distils ; and further, that this result obtains even with very minute quantities of arsenious acid.   With strong hydrochloric acid (containing upwards of 20 per cent. of real acid), anhydrous chloride collects at the bottom of the receiver distinct from the other products.   But with acid below 20 per cent., the chloride is found in the distillate in the state of solution.

It may appear singular that chloride of arsenic should so readily distil over with a liquid which boils at 230° F., when its own boiling-point is 274°.   This apparent anomaly, however, is explained by the fact, that the chloride distils freely at a temperature very much below its point of ebullition.

In evidence of the extreme facility with which arsenious acid yields the chloride when heated with hydrochloric acid, we may mention one or two experiments.

Two-tenths of a grain of arsenious acid were heated in a distilling apparatus with 550 grains of hydrochloric acid, specific gravity 1·100; when one-twentieth of the liquid had distilled over, the distillate was tested with sulphuretted hydrogen : a decided precipitate of sulphide of arsenic separated.

In another experiment one grain of arsenious acid was distilled with 550 grains of the same hydrochloric acid.   A piece of copper ribbon placed in the neck of the flask became coated with a crust of metallic arsenic as soon as the liquid began to boil. One-tenth part of the liquid was allowed to distil ; and on testing one-third of the distillate with sulphuretted hydrogen, the presence of arsenic was clearly indicated.   Another third of the distillate was subjected to Reinsch's process, which also gave unequivocal proof of the existence of arsenic.   The separation of

metallic arsenic in Reinsch's process appears to depend almost entirely on the ready production of chloride of arsenic, which is instantly decomposed on contact with copper.

As the formation and escape of chloride of arsenic appear therefore to occur with certainty when arsenious and hydrochloric acids are heated together, it became interesting to inquire whether similar results would obtain in presence of organic matters.

Half a grain of arsenious acid was mixed with an ounce of porridge, and an ounce of hydrochloric acid being added, the mixture was distilled. The moment the mixture began to boil, a slip of copper ribbon previously placed in the neck of the flask was coated with metallic arsenic. Nearly an eighth part of the mixture was distilled over, one-half of which gave with sulphuretted hydrogen the well-known effect of a solution of arsenic.

Again, two-tenths of a grain of AsO^3 were mixed with an ounce of porridge and three-fourths of an ounce of HCl, and the mixture distilled as before. Three drachms of the distillate, when tested by Marsh's process, afforded brilliant stains of metallic arsenic. The distillate gave also a yellow precipitate with sulphuretted hydrogen.

Similar experiments were performed with cow's liver and with common broth. The results were equally satisfactory.

The foregoing experiments plainly indicate the availability of the chloride of arsenic in medico-legal investigations for the separation of the metal from animal and vegetable matters; and we are induced to think that its production by the direct agency of hydrochloric acid will be found in practice to be more convenient than by distillation with sulphuric acid and common salt, as recommended by Dr. Fyfe. It appears to be peculiarly suitable for the preparation of the liquid to be subjected to Marsh's process. The chief precaution to be observed is to employ the hydrochloric acid in sufficient quantity and of full strength.

Several experiments were likewise made with sulphide of arsenic. It is slowly decomposed by concentrated hydrochloric acid, with the formation of chloride of arsenic; but the distillate is very apt to be contaminated with sulphide of arsenic, reproduced by the secondary action of the sulphuretted hydrogen disengaged at the same time. As common orpiment, however, usually contains arsenious acid, the present process seems also applicable to those cases in which this substance may occur.

We are at present engaged with the examination of some singular compounds of chloride of arsenic.

LVIII. *On a simple Geometrical Problem illustrating a conjectured Principle in the Theory of Geometrical Method. By* J. J. SYLVESTER, *Barrister-at-Law*[*].

THE following theorem deserves attention as illustrating a principle of geometrical method which will be presently adverted to. It is curious, also, from the fact of its solution being by no means so obvious and self-evident as one would expect from the extreme simplicity of its enunciation. It appeared, and for the first time, it is believed, at the University of Cambridge about a twelvemonth back, where it excited considerable attention among some of the mathematicians of the place. The proposition, as originally presented, was merely to prove that if ABC be a triangle, and if AD and BE drawn bisecting the angles at A and B and meeting the opposite sides in D and E be equal,
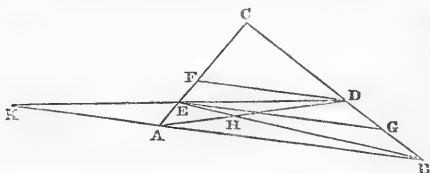
then the triangle must be isosceles. It is particularly noticeable that all the geometrical demonstrations yet given of this theorem are indirect. Thus the first and simplest (communicated to me by a promising young geometrician, Mr. B. L. Smith of Jesus College, Cambridge), was the following :—Assume one of the angles at DAB to be greater than the corresponding angle EBA; it can easily be shown that, upon this supposition, D will be higher up from AB than E ; so that if DF and EG be drawn parallel to AB, DF will be above EG; it is then easily shown that DF = AF, EG = BG, and consequently DF and AF are each respectively less than EG and BG ; and also DFA, which is the supplement of twice DAB, will be less than EGB, which is the supplement of twice FBA; from which it is readily inferred, by an easy corollary to a proposition of Euclid, that DA will be less than FB, whereas it should be equal to it ; so that neither of the half angles at the base can be greater than the other, and the triangle is proved to be isosceles. Another and independent demonstration by the writer of this article is less simple, but has the advantage of lending itself at once to a considerable generalization of the theorem as proposed. Assuming, as above, that DAB is greater than EBA, it is easily seen that DE produced will cut BA at K on the side of it : also if

* Communicated by the Author.

AD and BE intersect in H, it is readily demonstrable, by a suitably constructed apparatus of similar triangles, that

$$AH : BH : : CE : CD.$$

But as HBA is less than HAB, AH is less than BH, and therefore CE is less than CD, and therefore CED is greater than CDE; that is to say, CAB less K is greater than CBA plus K, and therefore DAB less K is greater than EBA, *i. e.* ADE is greater than ABE, and therefore the perpendicular from A upon DE is greater than that from E on AB, which is easily proved to be absurd. Hence, as before, the triangle is proved to be isosceles. This proof, it is obvious, remains good for *all cases* in which EB and DA, drawn on either side of the base, divide the angles at the base proportionally, provided that these lines remain equal, and make positive or negative angles with the base not *less* than one-half of the respective corresponding angles which the sides of the triangle are supposed to make with it. The analytical solution of the question, as might be expected, extends the result still further. To obtain this, let

$$BAC = n \cdot BAD, \qquad ABC = n \cdot ABE,$$

(n) for the present being any numerical quantity, positive or negative; calling $BAC = 2n\alpha$, $ABC = 2n\beta$, we readily obtain, by comparison of the equal dividing lines with the base of the triangle,

$$\frac{\sin(2n\alpha + 2\beta)}{\sin 2n\alpha} = \frac{\sin(2n\beta + 2\alpha)}{\sin 2n\beta},$$

or

$$\frac{\sin(2n\alpha + 2\beta)}{\sin(2n\beta + 2\alpha)} = \frac{\sin 2n\alpha}{\sin 2n\beta};$$

and by an obvious reduction,

$$\frac{\tan(n-1)(\alpha - \beta)}{\tan n(\alpha - \beta)} = \frac{\tan(n+1)(\alpha + \beta)}{\tan n(\alpha + \beta)}.$$

When this equation is put under an integer form, it is of course satisfied by making $\alpha = \beta$; on any other supposition than $\alpha = \beta$ it evidently cannot be satisfied by admissible values of the angles for any value of n between $+1$ and $+\infty$; for on that supposition, since $(\alpha - \beta)$ and $(\alpha + \beta)$ are each less than $\frac{180}{2n}$, the first side of the equation will be necessarily a proper fraction and positive; but the second side, either a positive improper fraction if $(n+1)(\alpha + \beta)$ be less, and a negative proper or a negative improper fraction if $(n+1)(\alpha + \beta)$ be greater than a right angle.

If n be negative, let it equal $-\nu$, then

$$\frac{\tan(\nu+1)(\alpha - \beta)}{\tan \nu(\alpha - \beta)} = \frac{\tan(\nu-1)(\alpha + \beta)}{\tan \nu(\alpha + \beta)};$$

and for the same reason as before, if ν lies between ∞ and 1, this equation cannot be satisfied. Hence the theorem is proved to be true for all values of n, except between $+1$ and -1. For these values it ceases to be true; in fact, for such values for any given values of $(\alpha - \beta)$ there will be always, as it may be easily proved, one or more values of $(\alpha + \beta)$; thus if $n = \dfrac{1}{2}$, the equation becomes

$$\frac{\tan 3\left(\dfrac{\alpha + \beta}{2}\right)}{\tan \dfrac{\alpha + \beta}{2}} = 1\,;$$

and if $n = -\dfrac{1}{2}$,

$$\tan \frac{\dfrac{3\alpha - \beta}{2}}{\tan \dfrac{\alpha - \beta}{2}} = 1,$$

showing that $\alpha + \beta = 90$ and $\alpha - \beta = \pm 90$ in these respective cases will afford a solution over and above the solution $\alpha = \beta$, which is easily verified geometrically*. It would be an interesting inquiry (for those who have leisure for such investigations) to determine for any given value of n between $+1$ and -1 the superior and inferior limit to the number of admissible values of $\alpha + \beta$ corresponding to any given value of $\alpha - \beta$†.

My reader will now be prepared to see why it is that all the geometrical demonstrations given of this theorem, even in the simplest case of all, viz. when $n = 2$, are indirect, I believe I may venture to say *necessarily* indirect. It is because the truth of the theorem depends on the necessary non-existence of real roots (between prescribed limits) of the analytical equation expressing the conditions of the question; and I believe that it may be safely taken as an axiom in geometrical method, that whenever this is the case no other form of proof than that of the *reductio ad absurdum* is possible in the nature of things. If this principle is erroneous, it must admit of an easy refutation in particular instances.

As an example, I throw out (not a challenge, but) an invitation to discover a direct proof, if such exist, of the following

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* In the first of these cases, if the base of the triangle is supposed given, the locus of the vertex is a right line and a circle; in the second case, a right line and an equilateral hyperbola.

† When $\pm n$ lies between $\dfrac{1}{2\iota - 1}$ and $\dfrac{1}{2\iota + 1}$ (ι being any positive integer),

it is easily seen that the superior limit must be at least as great as (ι).

geometrical theorem, as simple a one as it is perhaps possible to imagine :—"To prove that if from the middle of a circular arc two chords be drawn, and the remoter segments of these chords cut off by the line joining the end of the arc be equal, the nearer segments will also be equal." The analytical proof depends upon the fact of the equation $x^2 + ax = b^2$ (where (a) is the given length of each segment, and (b) the length of the chord of half the given arc) having only one admissible root ; and if the principle assumed or presumed to be true be valid, no other form of pure geometrical demonstration than the *reductio ad absurdum* should be applicable in this case. For the converse case, where the nearer segments are given equal, the reducing equation is $a(a+x) = b^2$, indicating nothing to the contrary of the possibility of there being a direct solution, which accordingly is easily shown to exist. The indirect form of demonstration, it may be mentioned, is sometimes liable to be introduced in a manner to escape notice. As, for instance, if it should be taken for granted in the course of an argument, that one triangle upon the same base and the same side of it as another triangle, and having the same vertical angle, must have its vertex lying on the same arc ; this would seem to be *immediately* true by virtue of the well-known theorem, that angles in the same circular segment are equal, but in reality can only be *inferred* from it indirectly by showing the impossibility of its lying outside or inside the arc in question. To go one step further, I believe it to be the case, that granted to be true all those fundamental propositions in geometry which are presupposed in the principles upon which the language of analytical geometry is constructed, then that the *reductio ad absurdum* not only is of necessity to be employed, but moreover in propositions of an affirmative character, never need be employed except when as above explained the analytical demonstration is founded on the impossibility or inadmissibility of certain roots due to the degree of the equation implied in the conditions of the question. If this surmise turn out to be correct, we are furnished with *a universal criterion for determining when the use of the indirect method of geometrical proof should be considered valid and admissible and when not**.

7 New Square, Lincoln's-Inn,
  October 4, 1852.

* If report may be believed, intellects capable of extending the bounds of the planetary system and lighting up new regions of the universe with the torch of analysis, have been baffled by the difficulties of the elementary problem stated at the outset of this paper, in consequence, it is to be presumed, of seeking a form of geometrical demonstration of which the question from its nature does not admit. If this be so, no better evidence could be desired to evince the importance of such a criterion than that suggested in the text.

LIX. *On Chemical Combination; and on the Amount of Heat produced by the Combination of several Metals with Oxygen. By* Thomas Woods, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Parsonstown, Sept. 1852.

THE two following papers were read at the Meeting of the British Association at Belfast. I was not present; but reports of some remarks made by Dr. Andrews there, as well as a paper in the Supplementary Number of this Journal for last July by Mr. Joule, increase my confidence by saying that one of the fundamental truths, the idea of which my theory suggested to me, and which I had been at great pains to prove, had been conjectured by the former, and had actually been proved although not published by the latter. On account of these remarks I think of course the more highly of the theory I propose: I have, however, no wish to take the merit either of the ideas or work of others; and lest I should unconsciously be treading on beaten ground, I will briefly state what I consider myself principally responsible for.

1st. By right of prior publication the *proof* that decomposition causes as much cold as the combination of the elements produced heat.

2nd. The idea that when two bodies are brought together at insensible distances, as in solution, their particles may be regarded as physically forming *one* body; and the mixture of these particles therefore subject to the same laws as if it were a simple body.

3rd. That every substance being composed of a definite amount of space as well as matter, the former as well as the latter must influence and be taken into account in chemical combination.

4th. That the amount of *heat* or relative volume in matter can neither be increased or diminished, whatever one body loses or gains being necessarily taken up or given off by some other.

And 5th. That therefore the supposition of attraction or repulsion between the particles of matter is unnecessary.

I believe I am also the first who endeavoured to obtain the amount of heat by the oxidizement of the metals in the moist way.

I have the honour, &c.,

Thomas Woods.

On Chemical Combination.

Dismissing all hypothetical ideas, and arguing from such phænomena only as can be demonstrated, I will endeavour to divest chemical action of any mysterious character or properties more

than those belonging to the ordinary changes in simple matter, and extend the laws which regulate the latter to combining and decomposing substances.

If two similar bodies unequally heated be placed together, one expands and the other contracts until an equal volume is attained by each.

If two bodies of equal volume be placed together, and then pressure be applied to one, the other becomes heated or expanded in proportion to the pressure or diminution of volume of the former.

If any substance expand, it deprives some other of heat or volume, as in the solution of salts, &c.

In the foregoing molecular changes of matter, it is evident, that, to whatever cause they may be attributed, *there are equal movements simultaneously occurring in opposite directions* (No. 1). Every substance is compressible, and has a certain specific gravity, proving that it is composed of particles, whether these be what are called atoms or otherwise; and that, because it has a *specific* gravity, these particles must be separated by a *definite* space or distance, which is always the same for the same body at like temperature and pressure; and at any distance from the zero of temperature, or absolute contact of particles, these particles must be at different distances in different bodies, for bodies expand differently when heated. It follows, then, that the space and matter which compose every substance must be related; or in other words, that *the distance between the particles of a body must be some function of, or have some dependence on, the nature of the matter composing it.* (No. 2.)

To these propositions may be added a third, that *particles form one body when they are at insensible distances.* (No. 3.)

Now before chemical action can take place, the bodies about to act must be brought together at insensible distances, that is, form one body; but if two substances of different kind form one body, and the distance between particles be some function of the nature of the matter (No. 2), it follows that the distance between the particles of the mixture of the two bodies cannot be the same as that of either. Hence the distances must change; and as (No. 1) every molecular change must be accompanied by an equal and opposite one, when the distances diminish, the distances between the particles of some other body increase or heat is produced. Chemical action may therefore be said to consist in an alteration of the distances between the particles of matter consequent on the change that is produced in the nature of the matter itself by the substitution of a mixture of two bodies for one; and as, if two substances be brought together from sensible to insensible distances, any alteration in the distance

of particles must be a lessening (for if it were an expansion, the bodies would not come to an insensible distance at all, or at least could not be brought nearer than the very bounds), the heat of chemical action is the necessary simultaneous, equal, and opposite molecular movement or expansion. It follows, also, that every atom, or, not to speak hypothetically, every combining particle of one element must necessarily unite with a particle of the other in the combination of the two bodies; for as the distance between the particles of the opposite kind is smaller than that between the particles of the same kind, the relation of the space to the nature of the matter could only be fulfilled by that arrangement.

If two simple bodies therefore combine, the distance between the elements of each compound particle being less than that between the particles of either body in the uncombined state, heat or expansion in some other substance must be produced; just as when the particles of iron, for example, contract or come together from pressure or any other cause, expansion in another substance is the result. I proved in the October Number of the Philosophical Magazine, 1851, that the opposite effect, or cold, is the consequence of the expansion among the particles in the decomposition of a compound.

The value of external circumstances in chemical combination may be estimated by the fact, that although at first the compound particles may absorb the expansion or heat produced by combination, yet other bodies in the end take it up; and this equal and opposite movement must be modified by them.

If it be true, then, that the distance between the particles of a body depends on the matter composing it (No. 2), and that chemical combination depends on a lessening of this distance, or rather consists therein, it follows that *affinity* results from the circumstance that when two bodies are mixed together the nature of the mixed matter requires a smaller distance between the particles than that of either body separately; and *elective* affinity is, that when *three* bodies are brought together at insensible distances, those two which require least distance between the particles always must unite. And as the opposite movement or expansion always accompanies the combination, the *heat* produced is a measure of the *affinity*; or if *heat* be produced by mixing a simple with a compound body, it shows that the former decomposed and united with an element of the latter. Notwithstanding that the decomposition of the latter produced cold, yet the combination that takes place at the same time produces more heat than counteracts that cold, because if that were not the case, the particles of the combining bodies would not have come more closely together than those of the decomposing one, and no

change at all would have taken place. For example : if potassium or zinc be placed in water, the metal, the oxygen and the hydrogen are at an insensible distance from each other ; but as the distance required between oxygen and zinc is less than between any other two of the elements, or between the particles of either of the elements by itself, the oxygen and zinc unite or come together, while the opposite movement or expansion is supplied partly by the expansion or separation of the oxygen and hydrogen, and partly by external bodies. Zinc can therefore decompose water ; but if copper be placed in water, as it does not produce as much heat by combining with oxygen as hydrogen does, or in other words, does not lie so closely to it in combination, no change is produced ; the water is not decomposed. If, however, oxide of silver in solution be substituted for the water, as copper produces more heat in combining witho xygen than silver does, the oxide of silver is decomposed, the oxygen uniting with the copper. Or if nitric acid and copper be mixed, as the last proportions of oxygen do not produce so much heat with the nitrogen as they do with the copper, the nitric acid is decomposed and oxide of copper formed.

Thus the heat produced is a measure of the so-called affinity of bodies ; for heat is but expansion among particles ; and whenever expansion occurs, contraction must be simultaneously going on ; that contraction in chemical action is a lessening of the distances between the combining particles ; and the greater this is, the greater is the heat.

The movements occurring in chemical combination are thus referred to the same causes, and made to differ in nothing from those occurring in simple matter when it contracts or expands. Perhaps the phænomena of what is called "latent heat" may more clearly express my meaning. I consider the particles of combining bodies to be similarly circumstanced with those of steam becoming condensed ; the distance between the particles in either case becomes less ; the opposite effect, or expansion, or heat in the case of steam, is called the latent heat ; in the case of the chemical action it is called the heat of chemical combination. They differ from each other in nothing except amount ; and this I endeavoured to account for in the January Number of the Philosophical Magazine, 1852.

To account, then, for chemical action, and the heat produced by it, we have only to admit the existence of two laws :—

1st. That the distance between the particles of bodies has some dependence on the matter composing them.

2nd. That any change in this distance in one is necessarily accompanied by an equal and opposite change in another.

As a postscript, I would say that the idea of attractions and

repulsions between particles of matter should be altogether dismissed as, to say the least, unnecessary; for in all the cases where they are supposed to exist, they must be acting equally and in an opposite direction, so annulling each other. If attraction be imagined to keep the particles of bodies together, an equal repulsion must be imagined to counteract the force exactly, or the particles should collapse. And if attraction be said to cause chemical combination, an equal repulsion must at the same time be supposed to act; for expansion or heat simultaneously occurs. I believe the particles of bodies are perfectly passive with respect to each other, and only move in expansion or contraction, as the opposite movement is at the same time determined by the law of relative volume or distance, as spoken of above.

I would refer for other particulars of the theory I offer to the January Number of the Philosophical Magazine, 1852; the present paper contains a mere outline. As I remarked in this paper, that theory pointed out the circumstance that bodies which produced most heat had the greatest affinity, or that, in fact, the heat produced by combination might measure the amount of that affinity, I made some experiments to test the truth of the opinion. In another paper which I submit with this one to the Meeting, I give the amount of heat which various simple bodies produce with oxygen; and it will be found that those bodies which produce more heat are also capable of taking oxygen from its combination with those producing less; and that if two bodies be combined, a third will cause their separation if it can produce more heat with either element than the other element does, and will have no effect if otherwise.

I will briefly sum up my opinions. I conceive that there is a mutual dependence or relation between the space and matter which compose a body; such relation causing the distance between the particles to be definite.

That, therefore, if the nature of the matter changes, the distance between its particles must also change.

That if two bodies be mixed or brought together at insensible distances, as in solution, they no longer are two but one body; and as they were dissimilar previously to being mixed, the one body they form must be dissimilar from either separately, and so the distance between the particles must be different. It must also be less; for if greater, the bodies could be brought nearer at sensible than insensible distances, and so would not form one body at all, which is contrary to our supposition. But as every molecular movement is accompanied by its opposite, this lessening of distance between combining particles is attended with expansion among others, and this expansion is the heat.

On the Amount of Heat produced by the Combination of several Metals with Oxygen.

The reasons spoken of in the preceding paper having led me to the opinion that the heat of chemical combination is not the result either of opposite electricities uniting, or the disengagement of any subtle fluid, or any affection of matter which is not met with in simple bodies, but the compensating, or accompanying and opposite movement among particles, whereby the loss of volume or distance between the particles uniting is balanced; and judging from the theory I advanced, that in a mixture of bodies those particles which could come the closest together would unite to the exclusion of others, or in other words, would have the greatest affinity,—I concluded that as the heat or expansion is equal, although opposite to the contraction of the combining bodies, it would be a measure of the affinity exerted between different substances. I accordingly endeavoured to ascertain the amount of heat liberated by the combination of several substances with oxygen, in order to find whether those which produced most heat were likewise the strongest bases. The method I adopted is partly founded on the inference I drew in the paper published in the October Number of the Philosophical Magazine, 1851, from the fact I there proved, "that the decomposition of a compound body gives rise to as much cold as the combination of its elements produces heat." I said (12) that "it might be made the means of determining the amount of heat produced by the combination of bodies, as the loss occasioned by their decomposition shows the gain by their combination."

Knowing, then, the amount of heat produced by the combination of hydrogen with oxygen, I knew that if water were decomposed a like amount would be absorbed; and therefore if any body were placed in contact with water, and could decompose it by joining with its oxygen, the amount of heat of such combination could be calculated by adding the heat absorbed by the decomposition to that marked by the thermometer. For instance, if potassium be placed in water, the oxygen unites with it, and a certain amount of heat is produced; but the decomposition of the water to supply the oxygen absorbs a certain amount also; the latter must be added to the former, and the sum is the quantity of heat the combustion of potassium in oxygen would produce. If the addition of sulphuric acid be necessary to make the metal continue the decomposition of the water, as in the case of zinc, then the heat produced by the combination of the oxide with the acid must be allowed for; and in cases (such as copper)

where the metal cannot decompose water, some other fluid must be substituted, as nitric acid; and the amount of heat absorbed by its decomposition allowed for in the final result.

I would remark as a preface to the following experiments, that although they are given as if an equivalent (oxygen being one) of each metal were dissolved in a quantity of fluid equal in value of being heated to 60 grains of water, no fixed weight of each was used; but according to circumstances, a portion, being weighed accurately, was dissolved, and the amount of heat being marked, the result was *calculated*. For instance, when experimenting with potassium or sodium, I always weighed whatever happened to come from the bottle, the pieces varying from half a grain to $2\frac{1}{2}$ grains. Zinc I used in larger pieces than mercury, because it was more quickly acted on, &c., but calculated what an equivalent of each would produce from knowing what the weight used developed in each case. The *fluid*, however, was in every instance the same in quantity: it amounted to 180 grains when nitric acid was used, and 207 when dilute sulphuric acid or water; that is, the menstruum, the vessel containing it, and the thermometer were, all taken together, equal in value of being heated to this quantity of water.

As an example of the exact steps followed, I will copy from my note-book one experiment with potassium. Having ascertained by previous experiments that the value of the glass tube which contained the fluid and the thermometer were equal in value to 27 grains, I placed in the tube 180 grains of water; its temperature 59° F., temperature of room 63° F. I rolled $1\frac{1}{2}$ grain of potassium in 10 grains of platina foil in order to make it sink in the water, and having placed it in the fluid, the water was decomposed, and the oxygen uniting with the potassium, the temperature rose to 70° F., that is, 11°. Calculating from this experiment, 1 grain of potassium would raise the temperature of 60 grains of water 25°·3 F., or 5 grains of potassium (equivalent to 1 grain of oxygen) would raise the temperature of 60 grains of water 126°·5 F. But to this must be added the heat absorbed by the decomposition of the water. According to Andrews, 1 grain of oxygen uniting with hydrogen would raise the temperature of 60 grains of water 126°·5 F.; therefore potassium must produce by the combination of an equivalent of oxygen exactly twice as much heat as hydrogen, or 253°·0 F. According, however, to Grassi, the combination of 1 grain of oxygen with hydrogen would raise the temperature of 60 grains of water 130° F.

Each of the following results is calculated from a *mean* of several experiments.

The amount of Heat produced by the Oxidizement of an equivalent of Sodium.

3 grains of sodium by dissolving in water raise the temperature of 60 grains 154° F., decomposition of water to supply 1 grain of oxygen absorbs 130°; therefore the 3 grains of sodium (or 1 equivalent) raise the temperature of 60 of water 154 + 130 = 284° F. by combining with oxygen.

Potassium.

5 grains of potassium raise the temperature of 60 grains of water by dissolving in it 126°·5 F.; add 130° for the heat absorbed by the decomposition of the water, and 256°·5 F. is the amount of heat liberated by the combustion of 1 equivalent of potassium.

Zinc.

The solution of 4 grains of zinc in sulphuric acid (dilute) produces heat sufficient to raise 60 grains of water 72° F. The combination of the oxide formed and the sulphuric acid developes in the same quantity of water 42°·2 F; this must therefore be subtracted from the 72°, as it is not due to the oxidizement of the zinc, and there then remains 29°·8; but to this quantity must be added 130° absorbed by the decomposition of the water, and 159°·8 F. is the amount of heat that the combustion of zinc would produce in 60 grains of water.

Copper.

Copper will not decompose water; nitric acid was therefore substituted for the dilute sulphuric acid. 3·96 grains, or 1 equivalent of copper, by dissolving in nitric acid raise the temperature of 60° grains of water 77°·22 F.; but in this is included the amount of heat produced by the combination of the oxide of copper with the nitric acid, which is 35° F. Subtract this sum, therefore, and 42°·22 remain; but as the decomposition of sufficient nitric acid to give one grain of oxygen would absorb as much heat as would lower the temperature of 60 grs. of water 30°·4 F., this must be added to the 42°·2 F., and we obtain 72°·6 F. as the amount of heat produced in 60 grains of water by the combustion of copper.

The manner in which I found that the decomposition of nitric acid absorbs the above amount of heat was by dissolving 4 grains of zinc in 180 grains of the acid; it liberated heat sufficient to raise 60 grains of water 171°·7 F. As this included the heat of combination of the oxide of zinc and nitric acid, 42°·2 must be subtracted, and there remains 129°·8 F.; but in the former experiment with the dilute sulphuric acid, it was shown that 4

grains of zinc being oxidized would raise the temperature of 60 grains of water 159°·8; therefore the difference between 159°·8 and 129°·8, equal to 30°·4 F., is absorbed by the decomposition of the acid.

Bismuth.

8·9 grains of bismuth by their solution in nitric acid develope sufficient heat to raise the temperature of 60 grains of water 80°·1 F. Subtract 36° for the heat of combination of the acid and oxide, and add 30°·4 for the absorption of heat by the decomposition of the acid, and the result is that 1 equivalent of bismuth produces by its combustion as much heat as would raise 60 grains of water 74°·5 F.

Lead.

12·7 grains, or 1 equivalent of lead, by its solution in nitric acid produce heat sufficient to raise the temperature of 60 grains of water 106° F., including 37° F. for the combination of the oxide with the acid; the latter being subtracted leaves 69°, to which must be added 30°·4 for the amount absorbed by the decomposition of the acid, and there remains 99°·4 F. as the amount of heat produced in 60 grains of water by the combustion of 1 equivalent of lead.

Mercury.

I could not ascertain satisfactorily the amount of heat produced by the oxidizement of mercury. Its solution in nitric acid is slow, and its equivalent number is high, and the combination it forms with the acid is not well ascertained. It is generally thought that when there is an excess of acid the salt formed is a *sub*-salt, Hg^2O being the base; if such be the case, 25 grains of mercury raise the temperature of 60 grains of water 50° F., including the combination of the oxide with the acid, and this produces sufficient heat to raise 60 grains of water 40°. The oxidizement alone produces $50° - 40° = 10°$ F.; to this sum add 30°·4 for the heat absorbed by the decomposition of the acid, and it leaves 40°·4 F. as the quantity of heat produced in 60 grains of water by the combustion of 1 equivalent of mercury.

Silver.

13½ grains, or 1 equivalent of silver, by its solution in nitric acid raise 60 grains of water 40°·5 F.; subtract 32° for the heat produced by the combination of the acid with the oxide of silver, and add 30°·4 for the heat absorbed by the decomposition of the acid, and 38°·9 F. is the number of degrees 1 equivalent of silver by its oxidizement would raise 60 grains of water.

The amount of heat produced by the oxidizement of iron could not be calculated with any certainty from its solution in dilute

sulphuric acid, as it requires a large amount to be dissolved to cause a perceptible rise of temperature. The heat it produces must be very nearly the same as that produced by hydrogen, as if it differed considerably, a small quantity of it would give rise to a large amount of heat, as its equivalent is small. The heat absorbed by the decomposition of the water very nearly balanced that produced by the combination of the iron with the oxygen. Iron produces only a very little more, if more at all, heat with oxygen than hydrogen does. When iron is dissolved in nitric acid, the peroxide is formed; and $2\frac{1}{3}$ grains, or 1 equivalent, (for $2\frac{1}{3}$ grains unite with 1 grain of oxygen), when dissolved in nitric acid, produce heat sufficient to raise 60 grains of water $102°$ F. As 1 equivalent, or 10 grains of peroxide of iron, by combining with acid produces heat sufficient to raise 60 grains of water $20°$ F., $3\frac{1}{3}$ grains must raise it $6°$; subtract this number from $102°$, there remains 96, and add $30\cdot4$ for decomposition of the acid, and $126°\cdot4$ F. is the quantity of heat produced by the oxidizement of iron.

Tin.

When tin is dissolved in nitric acid, $3\frac{1}{2}$ grains take 1 grain of oxygen to form the peroxide, and raise the temperature of 60 grains of water $105°$ F. If the combination of the peroxide with the acid produces the same amount of heat as the peroxide of iron does, $6°$ must be subtracted; there remains $99°$; add $30\cdot4$ for decomposition of acid, and $129°\cdot4$ F. is the number of degrees the oxidizement of tin would raise 60 grains of water.

The following table gives at a glance the number of degrees Fahrenheit that one equivalent of each metal, by its combination with oxygen (1 grain), would raise 60 grains of water. In the second column are the results that Andrews arrived at for such of the metals as he has experimented with by directly burning them in oxygen, calculated to the same standard as my own, viz. the quantity of heat their combustion would produce in 60 grains of water.

Name of metal.	Amount of heat produced by the combination of an equivalent of each with 1 grain of oxygen in 60 grains of water.	The same according to Andrews.
Sodium	284 F.	
Potassium ...	256·5	
Zinc	159·8	160·8 F.
Tin	129·6	126·9
Iron	126·4	124
Lead	99·4	
Bismuth......	74·5	
Copper	72·6	71·8
Mercury	40·4	
Silver.........	38·9	

The length to which this paper has run prevents me remarking on the foregoing table as fully as I would wish. It will be, however, seen that those metals which are capable of displacing others from neutral solutions produce more heat by their combination with oxygen; or, according to the view I take of chemical combination, require less distance between their particles and those of oxygen than do the other metals. Thus when a salt of silver in solution is poured on copper, oxygen, silver and copper are brought together at an insensible distance, mechanically for the first instant forming one body, their particles lying together, perfectly passive with respect to each other; but as we deduce from experiment that oxygen and copper lie more closely together than oxygen and silver, the particles of the oxygen and copper are exactly in the same predicament as a heated body would be in conjunction with a colder one, the particles being separated from one another to a distance greater than natural, so to speak, for the mean temperature; these particles therefore move together or contract just as those of a heated body would do; and the particles of oxygen and silver which may represent the colder body separate or expand to supply the opposite movement.

It will be seen I do not attempt to explain *why* copper and oxygen lie more closely together than silver and oxygen in combination. I merely say, that as their uniting is accompanied by a greater expansion, or heat, in other bodies, and as that expansion may be taken as equal and opposite to the contraction between the uniting bodies, those substances producing most heat must lie more closely together; and that therefore all such hypothetical ideas as electricities, subtle fluids, undulations, &c. may be discarded in accounting for the heat of chemical combination; and the movements immediately concerned in producing it may be looked on as being in nothing different from those where heat is given out from a simple body whose temperature is more elevated than surrounding ones, except in this particular, that a simple body whose temperature is raised loses its volume to other bodies by an approximation of its own or similar particles, but in chemical combination it is the approximation of diverse particles moving to unite.

LX. *On the Temperature of a Bismuth and Antimony Joint during the passage of an Electrical Current.* By RICHARD ADIE, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your Journal of 1st October, Dr. Tyndall has cited the experiments of M. Lenz in proof of the power of a feeble galvanic current to reduce the temperature of a bismuth and anti-

mony joint below that of the surrounding atmosphere. In the notices of Lenz's experiments, I can find no allowance made for the reduction of temperature by evaporation. Now in repeating some of them, I have found a hygrometric effect to the extent of 1 and 1·5 of Centigrade; and I conceive that the omission of the correction required for evaporation in a drier atmosphere may be taken for a reduction of temperature by electricity. But in my experiments I have never met a fact which in the least encourages the view that electricity reduces temperatures; and as it is exceedingly hostile to all that is hitherto known of the relation of heat to electricity, it seems to me to call for a thorough confirmation by the researches of others before it can be received as a fact. M. Becquerel, as I have already stated, has said that he could not reduce temperatures by electricity. When I first met with the account of this supposed discovery, I looked forward to valuable results flowing from it; and the cross designed by an experimenter on the Continent appeared to supply strong evidence in confirmation of M. Peltier's views; I therefore resolved to examine thoroughly this cross, being full of hope from it; but after spending a long time, I found the true explanation of its action on the galvanometer to be based on well-known principles, which need not occupy space in a repetition here.

Yours very respectfully,

Liverpool, October 4, 1852.   RICHARD ADIE.

LXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 13, 1852. THE Reply of the President and Council to a Letter addressed to them by the Secretary of State for Foreign Affairs, on the subject of the cooperation of different Nations in Meteorological Observations. Communicated by direction of the President and Council.

Somerset House, 10th May 1852.

SIR,—I have the honour to acknowledge the receipt of your letter of March the 4th, transmitting, by direction of the Earl of Malmesbury, several documents received from foreign governments in reply to a proposal made to them by Her Majesty's Government, for their cooperation in establishing a uniform system of recording meteorological observations, and requesting the opinion of the President and Council of the Royal Society in reference to a proposition which has been made by the Government of the United States, respecting the manner in which the proposed cooperation should be carried out.

Having submitted your letter with its enclosures to the Presi-

dent and Council of the Royal Society, I am directed to convey to you the following reply.

With reference to the subject of well-directed and systematically conducted meteorological observations generally, and to the encouragement and support to be given to them by the governments of different countries, the President and Council are of opinion that they are highly deserving of much consideration, not only for their scientific value, but also on account of the important bearing which correct climatological knowledge has on the welfare and material interests of the people of every country.

With reference to the proposal for the establishment of a uniform plan in respect to instruments and modes of observation, the President and Council are not of opinion that any practical advantage is likely to be obtained by pressing such a proposition in the present state of meteorological science. Most of the principal governments of the European Continent, as Russia, Prussia, Austria, Bavaria and Belgium, have already organized establishments for climatological researches in their respective states, and have placed them under the superintendence of men eminently qualified by theoretical and practical knowledge, and whose previous publications had obtained for them a general European reputation. Such men are Kupffer, Dove, Kreil, Lamont and Quételet; under whose direction the meteorological observations in the above-named countries are proceeding; the instruments have been constructed under their care, and the instructions drawn up and published by them under the sanction of their respective governments. The observations as they are made are sent to them, are reduced and coordinated under their superintendence, and are published at the expense of the governments. Every year is now producing publications of this nature in the countries referred to, and by the rapid intercommunication of these, the results of the experience of one country and the modifications and improvements which experience may suggest, become quickly known to all. To call on countries already so advanced in systematically conducted meteorological observations to remodel their instructions and instruments, with a view of establishing uniformity in these respects, would probably, if pressed, elicit from other governments also the reply which Her Majesty's Government have received from Prince Schwarzenberg, conveyed in the Earl of Westmoreland's letter to Viscount Palmerston, viz. the transmission of a copy of the instructions which have been given to the Meteorological Observatories, forty-five in number, in the Austrian dominions, and a reference to the results obtained at those observatories, which are stated to be in regular course of publication.

In an earlier stage, when these establishments were either forming or were only in contemplation, it was considered that advantage might arise from a discussion of the objects to be principally kept in view, and of the instruments and methods by which these might be most successfully prosecuted. For this purpose, a conference was held at Cambridge, in England, in 1845, which was attended by many of the most distinguished Meteorologists in Europe, and

amongst them by all the gentlemen whose names are above stated, and who were expressly sent by their respective governments. The impulse communicated by this assemblage was without doubt highly beneficial, and the influence of the discussions which took place may perhaps be traced in some of the arrangements under which the researches in different countries are now proceeding ; but in the stage to which they have advanced, it may be doubted whether any measures are likely to be more beneficial than those which would increase the facilities of a cheap and rapid intercommunication of the results of the researches which are in progress.

With reference " to the suggestions made by the scientific men of the United States," the proposition of Lieutenant Maury, to give a greater extension and a more systematic direction to the meteorological observations to be made at sea, appears to be deserving of the most serious attention of the Board of Admiralty. In order to understand the importance of this proposition, it will be proper to refer to the system of observations which has been adopted of late years in the navy and merchant service of the United States, and to some few of the results to which it has already led. Instructions are given to naval captains and masters of ships, to note in their logs the points of the compass from which the wind blows, at least once in every eight hours : to record the temperature of the air, and of the water at the surface, and when practicable, at considerable depths of the sea : to notice all remarkable phenomena which may serve to characterize particular regions of the ocean, more especially the direction, the velocity, the depths and the limits of the currents : special instructions also are given to whalers, to note down the regions where whales are found, and the limits of the range of their different species. A scheme for taking these observations regularly and systematically, was submitted by Lieut. Maury to the Chief of the Bureau of Ordnance and Hydrography, in 1842, and instantly adopted : detailed instructions were given to every American shipmaster, upon his clearing from the Custom House, accompanied by a request that he would transmit to the proper office, after his return from his voyage, copies of his logs, as far at least as they related to these observations, with a view to their being examined, discussed and embodied in charts of the winds and currents, and in the compilation of sailing directions to every part of the globe. For some years the instructions thus furnished received very little attention, and very few observations were made or communicated ; the publication, however, in 1848, of some charts, founded upon the discussion of the scanty materials which had come to hand or which could be collected from other sources, and which indicated much shorter routes than had hitherto been followed to Rio and other ports of South America, was sufficient to satisfy some of the more intelligent shipmasters of the object and real importance of the scheme, and in less than two years from that time it had received the cordial cooperation of the masters of nearly every ship that sailed. At the present time there are nearly 1000 masters of ships who are engaged in making these observations ; they receive freely in return the

charts of the winds and currents, and the sailing directions which are formed upon them, corrected up to the latest period.

Short as is the time that this system has been in operation, the results to which it has led have proved of very great importance to the interests of navigation and commerce. The routes to many of the most frequented ports in different parts of the globe have been materially shortened, that to St. Francisco in California by nearly one third : a system of southwardly monsoons in the equatorial regions of the Atlantic and on the west coast of America has been discovered; a vibratory motion of the trade-wind zones, and with their belts of calms and their limits for every month of the year, has been determined : the course, bifurcations, limits and other phenomena of the Great Gulf-stream have been more accurately defined, and the existence of almost equally remarkable systems of currents in the Indian Ocean, on the coast of China, and on the North-western coast of America and elsewhere has been ascertained : there are, in fact, very few departments of the science of meteorology and hydrography which have not received very valuable additions ; whilst the more accurate determination of the parts of the Pacific Ocean, where the sperm-whale is found (which are very limited in extent), as well as the limits of the range of those of other species, has contributed very materially to the success of the American whale fishery, one of the most extensive and productive of all their fields of enterprise and industry.

The success of this system of cooperative observations has already led to the establishment of societies at Bombay and Calcutta, for obtaining, by similar means, a better knowledge of the winds, currents, and the course of the streams of the Indian seas.

But it is to the government of this country that the demand for cooperation, and for the interchange of observations, is most earnestly addressed by the government of the United States ; and the President and Council of the Royal Society express their hope that it will not be addressed in vain. We possess in our ships of war, in our packet service and in our vast commercial navy, better means of making such observations, and a greater interest in the results to which they lead, than any other nation. For this purpose, every ship which is under the control of the Admiralty should be furnished with instruments properly constructed and compared, and with proper instructions for using them : similar instructions for making and recording observations, as far as their means will allow, should be sent to every ship that sails, with a request that the results of them be transmitted to the Hydrographer's Office of the Admiralty, where an adequate staff of officers or others should be provided for their prompt examination, and the publication of the improved charts and sailing directions to which they would lead ; above all, it seems desirable to establish a prompt communication with the Hydrographer's Office of the United States, so that the united labours of the two greatest naval and commercial nations of the world may be combined, with the least practicable delay, in promoting the interests of navigation.

The President and Council refer to the documents which have been submitted to them, and more especially to the "Explanations and Sailing Directions to accompany wind and current charts" prepared by Lieutenant Maury, for a more detailed account of this system of cooperative observations, and of the grounds upon which they have ventured to make the preceding recommendations.

(Signed)   S. HUNTER CHRISTIE, Sec. R.S.

H. U. Addington, Esq.

"Second Appendix to a paper entitled 'Discovery that the Veins of the Bat's Wing (which are furnished with valves) are endowed with rythmical contractility.'" By T. Wharton Jones, Esq., F.R.S. &c.

The author states that, from a microscopical examination of the blood-vessels and circulation in the ears of the long-eared bat, he has ascertained that, different from what he had discovered to be the case in the wings, the veins of the ears are unfurnished with valves, and are not endowed with rythmical contractility, and that the onward flow of blood in them is consequently uniform.

May 27.—The reading of Mr. Huxley's paper, "Upon the Morphology of the Cephalous Mollusca, as illustrated by the Anatomy of certain Heteropoda and Pteropoda," commenced at the last meeting, was resumed and concluded.

In the present memoir the author endeavours to determine, upon anatomical and embryological grounds, the true homologies of the different organs of the Cephalous Mollusca, and thence to arrive at some idea of the archetypal form, as definite modifications of which the existing molluscous forms may be considered to have arisen.

The Pelagic Heteropoda and Pteropoda, from their small size and extreme transparency, are peculiarly favourable subjects for the anatomical part of this investigation, and it is from a detailed examination of those systems of organs which are of importance for the purpose that the author deduces the following conclusions:—

1. In the *Heteropoda* the intestine is bent towards the dorsal or *hæmal* side in consequence of the development behind the anus of the visceral "hernia," which is therefore called a *post-abdomen.*

2. In the *Heteropoda*, the "foot," in its most perfect condition, consists of three portions, a *propodium, mesopodium* and *metapodium.*

3. The *Heteropoda* are more or less prosobranchiate, the degree depending upon the amount of development of the post-abdomen.

4. In the *Pteropoda* the intestine is bent towards the ventral or *neural* side, in consequence of the development of the visceral "hernia" in front of the anus. It is therefore called an *abdomen.*

5. In the *Pteropoda*, the foot, besides the parts mentioned above, possesses an additional appendage, the *epipodium,* which forms the expanded wing characteristic of the group.

6. The *Pteropoda* are opisthobranchiate, prosobranchiate, or intermediate in character, according to the degree of development of the *abdomen.*

The *Heteropoda* and *Pteropoda*, then, may be considered to represent two opposite phases of the modification of the molluscous archetype.

In the second part of the paper, the author endeavours, by carefully collating the known facts of the development of the Mollusca, to ascertain (*a*) the primary form of all cephalous Mollusca, and (*b*) the mode in which, in the course of development, this embryonic form becomes metamorphosed into the adult form; in order, if possible, to account, on the safe basis of ascertained morphological laws, for the peculiar modifications of structure which have been found, anatomically, to obtain among the Heteropoda and Pteropoda.

He finds that it is possible not only to deduce the structure of the Heteropoda and Pteropoda from a simple and symmetrical archetype by such morphological laws, but that all the cephalous Mollusca fall under one or other of the great types of which these have been taken as exemplifications.

After a discussion of the various theories of the homology of the organs of cephalous Mollusca proposed by Lovèn, Leuckart, &c., the following general conclusions are set forth :—

1. The cephalous Mollusca are all organized after the same fundamental form or archetype.

2. The arrangement of the systems of organs within this archetype is essentially the same as in the Vertebrata and Annulosa; that is to say, supposing the digestive system to form the axis of the body, the nervous centre lies on one side of that axis; the blood-vascular centre upon the opposite; and furthermore, the archetype is symmetrical with regard to a longitudinal vertical plane, passing through these three.

3. The *molluscous* archetype differs from the *vertebrate* in the circumstance—1, that the mouth opens upon the neural surface; 2, that the embryo commences its development upon the hæmal side.

It differs from the *articulate* archetype in the latter circumstance, and from both in the fact, that the proper appendicular system (represented by the epipodium) is almost rudimentary, and that the locomotive function is mainly performed by a development of the neural surface.

4. The process of concentration and fusion of parts by which the principal modifications are produced among the Vertebrata and Articulata, seems almost absent in the Mollusca; the changes among them being produced by an asymmetrical development of the primarily symmetrical archetype, a process comparatively rare among the Articulata and Vertebrata.

5. The part thus asymmetrically developed is invariably a portion of the hæmal surface, and may be called an *abdomen* or a *post-abdomen*, according as it is placed before or behind the anus.

6. The intestine is found to be bent in two directions among the Mollusca, hæmad or neurad, and these flexures correspond with the development of a post-abdomen or abdomen, respectively.

7. The process of development demonstrates that the Tectibranchiata, Nudibranchiata and Pectinibranchiata (in part at least) belong

to the former division, and that the Cephalopoda and Pulmonata belong to the latter.

8. Anatomical evidence shows that the Heteropoda have a hæmal flexure of the intestine, the Pteropoda a neural flexure; and it is almost certain that when their development is traced, the former will be found to have a post-abdomen, the latter an abdomen; there will then be two great divisions of the cephalous mollusca.

a. Those which develope an abdomen:—*Cephalopoda, Pteropoda, Pulmonata.*

b. Those which develope a post-abdomen:—*Heteropoda, Pectinibranchiata, Tectibranchiata, Nudibranchiata.*

9. Prosobranchism and Opisthobranchism may occur as secondary results of either course of development.

10. The principal nervous centres are similar in number and position throughout, and differ only in their arrangement and degrees of concentration. The amount of the latter does not correspond with the complexity of organization of the mollusk, but rather the reverse.

11. The organization of the vascular system is equally uniform; its completeness or incompleteness is no mark of complexity or simplicity of the rest of the organization.

12. The cephalous Mollusca are characterized by the possession of a peculiar organ, the dentigerous " tongue," whose mode of action resembles that of a chain-saw.

13. The locomotive apparatus, when fully developed, consists of four parts, the propodium, mesopodium, metapodium and epipodium. These parts are least modified in such mollusks as Atlanta or Pneumodermon; most altered and disguised in such as Cleodora or Octopus.

14. The term " mantle" should be confined to the surface of the *abdomen* or *post-abdomen*, and to the prolonged edges of that surface.

15. It is of great importance to recollect that the " shells" are probably not homologous organs in all the different forms of mollusks.

The shells of Sepia, Spirula (?), Limax, Clausilia and Helix are developed in the thickness of the mantle.

The shells of Nautilus (?), Pectinibranchiata, &c., are developed from the surface of the mantle by a quite distinct process.

Certain curious differences appear to follow the externality or internality of the shell.

An external shell in a mollusk with a hæmal flexure, *e. g.* Atlanta, has its columellar axis *below* the aperture.

An external shell in a mollusk with a neural flexure, *e. g.* Nautilus, has its columellar axis *above* the aperture.

An internal shell in a mollusk with a neural flexure, has its columellar axis *below* the aperture, *e. g.* Spirula, Clausilia, Helix.

In the course of the memoir the author incidentally introduces a number of new, and, as he believes, important facts, with regard to the nervous, circulatory and urinary systems; and describes at length the mechanism of the " tongue" and an organ similar to the " crystalline style" of bivalves, found in the Strombidæ.

The following paper was also read :—

"On the Change of Refrangibility of Light." By George G. Stokes, Esq., M.A., F.R.S., Lucasian Professor of Mathematics, Cambridge.

The author was led into the researches detailed in this paper by considering a very singular phenomenon which Sir John Herschel had discovered in the case of a weak solution of sulphate of quinine, and various other salts of the same alkaloid. This fluid appears colourless and transparent, like water, when viewed by transmitted light, but exhibits in certain aspects a peculiar blue colour. Sir John Herschel found that when the fluid was illuminated by a beam of ordinary daylight, the blue light was produced only throughout a very thin stratum of fluid adjacent to the surface by which the light entered. It was unpolarized. It passed freely through many inches of the fluid. The incident beam, after having passed through the stratum from which the blue light came, was not sensibly enfeebled nor coloured, but yet it had lost the power of producing the usual blue colour when admitted into a solution of sulphate of quinine. A beam of light modified in this mysterious manner was called by Sir John Herschel *epipolized.*

Several years before Sir David Brewster had discovered in the case of an alcoholic solution of the green colouring matter of leaves a very remarkable phenomenon, which he has designated as *internal dispersion.* On admitting into this fluid a beam of sunlight condensed by a lens, he was surprised by finding the path of the rays within the fluid marked by a bright light of a blood-red colour, strangely contrasting with the beautiful green of the fluid itself when seen in moderate thickness. Sir David afterwards observed the same phenomenon in various vegetable solutions and essential oils, and in some solids. He conceived it to be due to coloured particles held in suspension. But there was one circumstance attending the phenomenon which seemed very difficult of explanation on such a supposition, namely, that the whole or a great part of the dispersed beam was unpolarized, whereas a beam reflected from suspended particles might be expected to be polarized by reflexion. And such was, in fact, the case with those beams which were plainly due to nothing but particles held in suspension. From the general identity of the circumstances attending the two phenomena, Sir David Brewster was led to conclude that epipolic was merely a particular case of internal dispersion, peculiar only in this respect, that the rays capable of dispersion were dispersed with unusual rapidity. But what rays they were which were capable of affecting a solution of sulphate of quinine, why the active rays were so quickly used up, while the dispersed rays which they produced passed freely through the fluid, why the transmitted light when subjected to prismatic analysis showed no deficiencies in those regions to which, with respect to refrangibility, the dispersed rays chiefly belonged, were questions to which the answers appeared to be involved in as much mystery as ever.

After having repeated some of the experiments of Sir David

Brewster and Sir John Herschel, the author could not fail to take a most lively interest in the phenomenon. The firm conviction which he felt that two portions of light were not distinguishable as to their nature otherwise than by refrangibility and state of polarization, left him but few hypotheses to choose between, respecting the explanation of the phenomenon. In fact, having regarded it at first as an axiom that dispersed light of any particular refrangibility could only have arisen from light of the same refrangibility contained in the incident beam, he was led by necessity to adopt hypotheses of so artificial a character as to render them wholly improbable. He was thus compelled to adopt the other alternative, namely, to suppose that in the process of internal dispersion the refrangibility of light had been changed. Startling as such a supposition might appear at first sight, the ease with which it accounted for the whole phenomenon was such as already to produce a strong probability of its truth. Accordingly the author determined to put this hypothesis to the test of experiment.

The experiments soon placed the fact of a change of refrangibility beyond all doubt. It would exceed the limits of an abstract like the present to describe the various experiments. It will be sufficient to mention some of the more remarkable results.

A pure spectrum from sunlight having been formed in air in the usual manner, a glass vessel containing a weak solution of sulphate of quinine was placed in it. The rays belonging to the greater part of the visible spectrum passed freely through the fluid, just as if it had been water, being merely reflected here and there from motes. But from a point about half-way between the fixed lines G and H to far beyond the extreme violet the incident rays gave rise to light of a sky-blue colour, which emanated in all directions from the portion of the fluid which was under the influence of the incident rays. The anterior surface of the blue space coincided of course with the inner surface of the vessel in which the fluid was contained. The posterior surface marked the distance to which the incident rays were able to penetrate before they were absorbed. This distance was at first considerable, greater than the diameter of the vessel, but it decreased with great rapidity as the refrangibility of the incident rays increased, so that from a little beyond the extreme violet to the end the blue space was reduced to an excessively thin stratum adjacent to the surface by which the incident rays entered. It appears therefore that this fluid, which is so transparent with respect to nearly the whole of the visible rays, is of an inky blackness with respect to the invisible rays more refrangible than the extreme violet. The fixed lines belonging to the violet and the invisible region beyond were beautifully represented by dark planes interrupting the blue space. When the eye was properly placed, these planes were of course projected into lines. The author has made a sketch of these fixed lines, which accompanies the paper. They may be readily identified with the fixed lines represented in M. Becquerel's map of the fixed lines of the chemical spectrum. The last line seen in a solution of sulphate of quinine appears to be the line next beyond the last represented in M. Becquerel's map. Under very favourable

circumstances two dusky bands were seen still further on. Several circumstances led the author to conclude that in all probability fixed lines might be readily seen corresponding to still more refrangible rays, were it not for the opacity of glass with respect to those rays of very high refrangibility.

It is very easy to prove experimentally that the blue dispersed light corresponding to any particular part of the incident spectrum is not homogeneous light, having a refrangibility equal to that of the incident rays, and rendered visible in consequence of its complete isolation; but that it is in fact heterogeneous light, consisting of rays extending over a wide range of refrangibility, and not passing beyond the limits of refrangibility of the spectrum visible under ordinary circumstances. To show this it is sufficient to isolate a part of the incident spectrum, and view the narrow beam of dispersed light which it produces through a prism held to the eye.

In Sir David Brewster's mode of observation, the beam of light, which was of the same nature as the blue light exhibited by a solution of sulphate of quinine, was necessarily mixed with the beam due merely to reflexion from suspended particles; and in the case of vegetable solutions, a beam of the latter kind almost always exists, to a greater or less degree. But in the method of observation employed by the author, to which he was led by the discovery of the change of refrangibility, the two beams are exhibited quite distinct from one another. The author proposes to call the two kinds of internal dispersion just mentioned *true internal dispersion* and *false internal dispersion*, the latter being nothing more than the scattering of light which is produced by suspended particles, and having, as is now perfectly plain, nothing to do with the remarkable phenomenon of true internal dispersion.

Now that the nature of the latter phenomenon is better known, it is of course possible to employ methods of observation by which it may be detected even when only feebly exhibited. It proves to be almost universal in vegetable solutions, that is, in solutions made directly from various parts of vegetables. When vegetable products are obtained in a state of isolation, their solutions sometimes exhibit the phenomenon and sometimes do not, or at least exhibit it so feebly that it is impossible to say whether what they do show may not be due to some impurity. Among fluids which exhibit the phenomenon in a high degree, or according to the author's expression are highly *sensitive*, may be mentioned a weak decoction of the bark of the horse-chestnut, an alcoholic extract from the seeds of the *Datura stramonium*, weak tincture of turmeric, and a decoction of madder in a solution of alum. In these cases the general character of the dispersion resembles that exhibited by a solution of sulphate of quinine, but the tint of the dispersed light, and the part of the spectrum at which the dispersion begins, are different in different cases. In the last fluid, for example, the dispersion commences somewhere about the fixed line D, and continues from thence onwards far beyond the extreme violet. The dispersed light is yellow, or yellowish orange.

In the case of other fluids, however, some of them sensitive in a

very high degree, the mode in which light is dispersed internally presents some very remarkable peculiarities. One of the most singular examples occurs in the case of an alcoholic solution of the green colouring matter of leaves. This fluid disperses a rich red light. The dispersion commences abruptly about the fixed line B, and continues from thence onwards throughout the visible spectrum and a little beyond. The dispersion is subject to fluctuations intimately connected with the singular absorption bands exhibited by this medium.

In order that a medium should be capable of changing the refrangibility of light incident upon it, it is not necessary that the medium should be a fluid, or a clear solid. Washed papers and other opake substances produce the same effect, but of course the mode of observation must be changed. The author has observed the change of refrangibility in various ways. It will be sufficient to mention here that which was found most generally useful, which he calls the method of observing by a *linear spectrum*. The method is as follows.

A series of prisms and a lens are arranged in the usual manner for forming a pure spectrum, but the slit by which the light enters, instead of being parallel, is placed in a direction perpendicular to the edges of the prisms. A linear spectrum is thus formed at the focus of the lens, consisting of an infinite succession of images of the slit arranged one after the other in the order of refrangibility, and of course overlapping each other to a certain extent. The substance to be examined is placed in the linear spectrum, and the line of light seen upon it is viewed through a prism held to the eye. In this way it is found that almost all common organic substances, such as wood, cork, paper, calico, bone, ivory, horn, wool, quills, feathers, leather, the skin of the hand, the nails, are sensitive in a greater or less degree. Organic substances which are dark-coloured are frequently found to be insensible, but, on the other hand, scarlet cloth and various other dyed articles are highly sensitive. By means of a linear spectrum the peculiar dispersion of a red light produced by chlorophyll, or some of its modifications, may be observed not only in a solution, but in a green leaf, or on a washed paper, or in a seaweed.

The highly sensitive papers obtained by washing paper with tincture of turmeric, or a solution of sulphate of quinine, or some other highly sensitive medium, display their sensibility in a remarkable manner when they are examined in a linear spectrum. In these cases, however, the paper produces a very striking effect when merely held so as to receive a pure spectrum formed in the usual manner, that is, with a slit parallel to the edges of the prisms. Such a paper may be used as a screen for showing the fixed lines belonging to the invisible rays, though they are not thus shown quite so well as by using a solution. The extraordinary prolongation of the spectrum seen when it is received on turmeric paper, has been already observed by Sir John Herschel, by whom it was attributed to a peculiarity in the reflecting power of that substance. Of course it now appears that the true explanation is very different.

A high degree of sensibility appears to be rather rare among inorganic compounds. Certain specimens of fluor spar, as is already known, give a copious internal dispersion of a deep blue light; but this is plainly due to some foreign ingredient, the nature of which is at present unknown. But there is one class of inorganic compounds which are very remarkable for their sensibility, namely, certain compounds of peroxide of uranium, including the ornamental glass called canary glass, and the natural mineral yellow uranite. In these compounds the dispersed light is found on analysis to consist of bright bands arranged at regular intervals. A very remarkable system of absorption bands is also found among these compounds, which is plainly connected with the system of bright bands seen in the spectrum of the dispersed light. The connection between the absorption and internal dispersion exhibited by these compounds is very singular, and is of a totally different nature from the connection which has been already mentioned as occurring in solutions of the green colouring matter of leaves.

There is one law relating to the change of refrangibility which appears to be quite universal, namely, that the refrangibility of light is *always lowered* by internal dispersion. The incident rays being homogeneous, the dispersed light is found to be more or less composite. Its colour depends simply on its refrangibility, having no relation to the colour of the incident light, or to the circumstance that the incident rays were visible or invisible. The dispersed light appears to emanate in all directions, as if the solid or fluid were self-luminous while under the influence of the incident rays.

The phenomenon of the change of refrangibility of light admits of several important applications. In the first place it enables us to determine instantaneously the transparency or opacity of a solid or fluid with respect to the invisible rays more refrangible than the violet, and that, not only for these rays as a whole, but for the rays of each refrangibility in particular. For this purpose it is sufficient to form a pure spectrum with sun-light as usual, employing instead of a screen a vessel containing a decoction of the bark of the horse-chestnut, or a slab of canary glass, or some other highly sensitive medium, and then to interpose the medium to be examined, which, if fluid, would have to be contained in a vessel with parallel sides of glass. Glass itself ceases to be transparent about the region corresponding to the end of the author's map, and to carry on these experiments with respect to invisible rays of still higher refrangibility would require the substitution of quartz for glass. The reflecting power of a surface with respect to the invisible rays may be examined in a similar manner.

The effect produced on sensitive media leads to interesting information respecting the nature of various flames. Thus, for example, it appears that the feeble flame of alcohol is extremely brilliant with regard to invisible rays of very high refrangibility. The flame of hydrogen appears to abound in invisible rays of still higher refrangibility.

By means of the phenomena relating to the change of refrangibility, the independent existence of one or more sensitive substances may frequently be observed in a mixture of various compounds. In

this way the phenomenon seems likely to prove of value in the separation of organic compounds. The phenomena sometimes also afford curious evidence of chemical combinations; but this subject cannot here be further dwelt upon.

The appearance which the rays from an electric spark produce in a solution of sulphate of quinine, shows that the spark is very rich in invisible rays of excessively high refrangibility, such as would plainly put them far beyond the limits of the maps which have hitherto been made of the fixed lines in the chemical part of the solar spectrum. These rays are stopped by glass, but transmitted through quartz. These circumstances render it probable that the phosphorogenic rays of an electric spark are nothing more than rays of the same nature as those of light, but which are invisible, and not only so, but of excessively high refrangibility. If so, they ought to be stopped by a very small quantity of a substance known to absorb those rays with great energy. Accordingly the author found that while the rays from an electric spark, which excite the phosphorescence of Canton's phosphorus, pass freely through water and quartz, they are stopped on adding to the water an excessively small quantity of sulphate of quinine.

At the end of the paper the author explains what he conceives to be the cause of the change of refrangibility, and enters into some speculations to account for the law according to which the refrangibility of light is always lowered in the process of internal dispersion.

LXII. *Intelligence and Miscellaneous Articles.*

REMARKS ON THE MECHANICAL EQUIVALENT OF HEAT.
BY A. F. KUPFFER.

WHEN a perpendicular wire fastened at its upper end is loaded at its lower end with a weight, it is expanded longitudinally to a certain degree. Let us, for example, imagine a wire whose length and radius (I assume that the section is a circle) are equal to unity stretched by the unity of weight, and call the extension which it thus suffers δ; we will call this quantity the elastic constant.

When the same wire has its temperature raised from the freezing to the boiling-point of water, it also suffers an expansion, which we will call a.

The quantity of heat which effects this expansion can only be determined comparatively; we may imagine a cylinder of water, whose height and radius at 32° are likewise equal to unity, and take the quantity of heat necessary to raise the temperature of this cylinder from 32° to 212° as unity. Then

$$m \,.\, S$$

is the quantity of heat necessary to raise the temperature of the above-mentioned wire from 32° to 212°, m representing the specific heat of the metal of which the wire is made, and S its specific weight compared with water.

Now since the extension which a wire suffers is equal to the forces

applied, it is evident that the value of a and δ afford us a means of comparing the expansive force of heat with the extending force of a weight; or in other words, these values enable us to determine the mechanical equivalent of heat. It must not here be forgotten, that heat acts equally on all sides like pressure; but Poisson has shown that a weight which extends a wire to the amount of δ, applied as pressure acting equally in all directions, would produce a linear expansion equal to $\frac{1}{2}\delta$. We have therefore to regard

$$\frac{2a}{\delta}$$

as the relation of the mechanical effect of the above-mentioned quantity of heat to the mechanical effect of a pound weight. In order to express this relation numerically, it is only necessary to know the elastic constant of any given substance, its specific heat, specific weight, and its expansion by heat.

I have determined* the elastic constants of several metals by means of the times of oscillation of wires 10 feet in length and 1 line in diameter, which were fastened at their upper ends, and carried at their lower ends a horizontal lever whose moment of inertia was known, and which was loaded with weights. This method, which admits of very great accuracy, gave me the following elastic constants (δ), to which I add the specific gravities (S) of the wires employed:—

	δ.	S.
Iron wire	0·00000001110	7·5536
Brass wire	2139	8·4760
Platinum wire . .	1269	20·9624
Silver wire	2854	10·4845

These metals have the following specific heats (m), and coefficients of expansion by heat (a) :—

	m.	a.
Iron	0·11379	0·001182
Brass	0·09391	0·001878
Platinum	0·03243	0·0008842
Silver	0·05701	0·001910

All these values must correspond with the equation

$$c \cdot m \cdot S \cdot \tfrac{1}{2}\delta = a,$$

in which c is the mechanical equivalent of the heat which is necessary to raise the temperature of the cylinder of water from $32°$ to $212°$, or the pressure expressed in pounds which this quantity of heat exerts.

If these values are inserted in the above equation, we obtain as the value of c in each case the following numbers :—

For iron wire	$c = 247800$
For brass wire	$c = 220600$
For platinum wire . .	$c = 205050$
For silver wire	$c = 223900$

* *Mém. d. Acad. St. Pétersb.* VI. Série, Sc. Math. et Phys., vol. v. pp. 233–302.

When with these values of c the expansions of the metals by heat are calculated, we obtain—

	a calculated.	a observed.
Iron	0·001070	0·001182
Brass	0·001909	0·001878
Platinum	0·000968	0·000854
Silver	0·001918	0·001910

A closer correspondence was not to be expected with numbers which have been determined by such different observers, and upon which certainly no slight influence is exercised by the particular condition of the metals experimented with. I am at the present time engaged in the determination of two of the values in question —the elastic constants and the coefficients of expansion by heat for the same wires—and hope then to arrive at more accurate results.

The pressure of 224325 Russian pounds acted upon the surface of π square inches, we have then for 1 square inch 71441 pounds, or more than 4327 atmospheres.

The mechanical equivalent of heat may also be expressed in another way. The above-mentioned metal cylinder is extended to the amount of δ by the gravitation of one pound; the weight $p = \frac{1}{\delta}$ would then lengthen it one inch; we may then express the elastic force of the cylinder by saying it raises the weight p one inch; for it holds the force p which has sunk one inch in equilibrium.

If the same cylinder is heated to 212°, it expands to the amount of a; and according to the above hypothesis, it would expand to the amount of $2a$ if the heat acted only in one direction like the weight p. The quantity of heat which causes this expansion is $w \cdot m\mathrm{S}$, when we represent by w the quantity of heat which is necessary to raise the temperature of a cylinder of water of a height and radius equal to unity from 32° to 212°. Hence it follows that

$$\frac{w \cdot m\mathrm{S}}{2a}$$

is the quantity of heat which would effect an extension of one inch; or as forces which bring about equal effects must themselves be equal, then

$$p = \frac{m \cdot w\mathrm{S}}{2a}.$$

But we have also

$$p = \frac{1}{\delta} \text{ and } \frac{m\mathrm{S}\delta}{2a} = c,$$

therefore $w = c$.

The quantity of heat necessary to heat the cylinder of water from 32° to 212° is then capable of raising 224325 Russian pounds one inch. This cylinder of water weighs 0·1134 of an English pound (at the temperature of the greatest density); the number of Fahrenheit degrees between the freezing and boiling-points of water is 180, and a Russian pound = 0·9028 of an English pound; we have then

$$\frac{c \cdot 0\cdot9028}{0\cdot1134 \cdot 180} = 9921$$

for the number of English pounds which the heat necessary to raise one English pound of water from $32°$ to $212°$ is capable of raising one inch.

Joule found by experiments on the heat produced by friction 10680. By experiments on the heat evolved by the compression of air he found 9876 and 9540. All these numbers agree tolerably well.— Poggendorff's *Annalen*, 1852, No. 6.

ON THE RAIN-WATER COLLECTED AT THE OBSERVATORY AT PARIS. BY M. BARRAL.

Up to the end of the seventeenth and beginning of the eighteenth centuries the atmosphere was reckoned amongst the few elements admitted at that time. In course of time the researches of Van Helmont, Hales, Mayow, Bergmann, Scheele and Lavoisier, led to the knowledge of the fact that the atmosphere is a mixture of oxygen and nitrogen. The later chemists set themselves to the task of determining these constituents with greater exactitude than was possible to their predecessors. Thus Cavendish, Davy, Marty and Berthollet showed that the air had the same composition in all climates. Gay-Lussac ascertained the same fact with regard to air obtained by him by means of an air-balloon from a height to which no one had previously attained. He, together with Humboldt, furnished considerable assistance to Lavoisier in his determinations. Afterwards, in 1822, Despretz made numerous analyses of the air and arrived at similar results. Lastly, Dumas, Boussingault and Regnault have carried the accuracy of the analyses much further by operating upon larger quantities of air; and it appears that posterity has nothing more to determine, except whether the ascertained composition of the atmosphere be constant, whether the causes, such as combustion, respiration, &c. which lessen its quantity of oxygen, are accurately compensated by the known sources of oxygen. The atmosphere contains also vapour of water and carbonic acid. It is not yet ascertained who first discovered the presence of the latter; Black proved, immediately after the discovery of carbonic acid, that the thin crust which is formed upon lime-water when exposed to the air, consists of carbonate of lime.

All the preceding statements referred to the atmosphere in a state of purity. But winds, storms, and the ascending current of air produced by inequality of temperature, bear dust and particles of water from the foam of the sea, with the air which has been in contact with the ground, into higher regions. Such is, for instance, the red rain, with which the philosophers of the seventeenth century, Wendelin, Descartes, Peiresc and Gassendi, occupied themselves to such an extent.

It was only towards the middle of the last century that it was felt that the causes of such variations must be ascertained by regularly continued observations. These were undertaken at first only with the view of determining how far from the point of their origin such perturbations extend themselves. The study of rain, which, as it

falls through all the strata of air lying below the clouds which produce it, becomes impregnated with any matters contained therein, was the first thing to which observers directed their attention.

Bergmann was the first who appeared in this sphere. He discovered nitric acid in rain. Then follow Brandes, Zimmermann, Liebig and Jones.

The most important result of M. Barral's investigations is, that the rain in every month contains so much nitric acid and ammonia that the quantities of both can be determined. The author's remarks upon the quantity of chlorides contained in the atmosphere are not less interesting; but in the determination of these he has had Brandes, Berzelius, Liebig, Chatin, Meyrac and Isidore Pierre, as predecessors.

Bergmann had stated that traces of nitric acid occurred in the atmosphere. Brandes, who investigated the air at Salzuffeln from month to month in 1825, stated that he found in it chloride of magnesium, chloride of sodium, sulphate and carbonate of lime, carbonate of potash, oxides of iron and manganese, and traces of ammonia, sulphuric acid, and animal and vegetable matters. As regards the occurrence of chloride of iron, oxide of manganese, and chloride of potassium, this has already been questioned by Liebig. Liebig, in the investigation of seventy-seven samples of rain-water, found nitric acid in ten samples after stormy rain, and traces in two only of the remaining sixty.

Bence Jones, in 1851, found nitric acid in the rain of London, Melburgh in Dorsetshire, and Clonakelty, by means of iodide of potassium paste, but did not determine its quantity.

In 1851, Barral commenced his investigations upon the rain collected partly upon the platform, and partly in the court of the Observatory at Paris.

The Commission has tried M. Barral's process, and found that his mode of determination was to be depended upon. The process itself is not further described. .

On the other hand, the following tables give the numbers which express the quantity of nitric acid in the rain. According to them, the rain is never equally loaded with nitric acid, and the quantities of nitric acid which fall with the rain upon one hectare of land are not in proportion to the quantity of rain. Calculated from the minimum, 31 kilogs. of nitrogen fall during the year upon one hectare of land in the neighbourhood of Paris.

Average contents of Rain-water from monthly determinations in the Udometers of the Observatory of Paris during the second half of the year 1851. *The numbers refer to one cubic metre of rain-water.*

	N.	N O5.	N H3.	Cl.	CaO.	MgO.	Total.
	grms.	grms.	grms.	grms.	grms.	grms.	grms.
July	4·67	6·01	3·77	3·88	9·02	...	24·80
August	9·44	20·20	4·42	2·89	8·68	...	38·31
September	11·95	36·33	3·04	2·39	7·16	...	51·04
October	4·46	5·82	1·08	1·84	2·43	...	13·29
November	4·64	9·99	2·50	2·64	4·26	...	21·51
December	15·01	36·21	6·85	0·00	7·36	...	52·54
Mean	8·36	19·09	3·61	2·27	6·48	2·12	33·57

The same determinations calculated to one hectare of land.

	N.	NO5.	NH3.	Cl.	CaO.	MgO.	Total.
	kilogs.	kilogs.	kilogs.	kilogs.	kilogs.	kilogs.	kilogs.
July	3·90	5·03	3·15	3·24	7·54	...	19·71
August	2·18	4·89	1·04	0·69	2·12	...	9·49
September	2·94	8·89	0·77	0·59	1·81	...	12·82
October	2·26	2·81	0·53	0·88	1·15	...	6·13
November	1·93	4·26	1·01	1·10	1·78	...	8·91
December	2·50	5·95	1·17	0·00	1·23	...	9·11
In 6 months ...	13·71	31·83	7·67	6·50	15·63	4·54	66·17

Barral then shows how much nitrogen falls as nitric acid and how much as ammonia. Of the 31 kilogs. of nitrogen, 9 belong to the ammonia and 22 to the nitric acid. For the separation of the nitric acid from the ammonia, the author has made use of a method invented by Peligot.

Bineau has lately determined the quantity of ammonia contained in the rain collected at Lyons, but not the quantity of nitric acid.— *Comptes Rendus*, vol. xxxiv. p. 824.

Meyrac has instituted similar investigations, and found that all rain contains chlorides ; the largest quantity of chloride of sodium found amounted to 2 centigrms. in the litre. This quantity is frequently contained in the water in autumn and winter, and in the first days of spring. It has always an alkaline reaction, and contains traces of iodine. The ammoniacal salt, which is contained in rain and snow-water, and which, according to M. Chatin, consists of carbonate, nitrate, and humate of ammonia, if first acidified with sulphuric acid and then brought in contact with carbonate of soda, evolves an empyreumatic odour. As this odour is produced by none of the above ammoniacal salts, it probably proceeds from other organic substances.—*Ibid.* vol. xxxiv. p. 715.

ON THE SALTS OF ANTIMONIC ACID. BY L. HEFFTER.

The results of this careful and accurate investigation are in part very remarkable and unexpected. The author has found that the proportion of oxygen in the base to that of the acid is only then as 1 to 5, when the antimoniates of the alkalies are heated to redness in an atmosphere of carbonic acid gas or of carbonate of ammonia, and treated with water, which extracts a little carbonate of the alkali. Even the crystalline antimoniate of soda, which has been obtained by the precipitation of a solution of antimoniate of potash by a soda salt, contains an excess of soda united with water, so that in this salt the oxygen of the soda to that of the acid is in the proportion of 1 to 4·6. By heating to redness it does not entirely lose its water, because the excess of soda retains the water, which it exchanges for carbonic acid when this is presented to it at a red heat. Something similar takes place with antimoniate of potash and with all other antimoniates which have been investigated ; M. Heffter

has succeeded in obtaining many of them in a distinctly crystallized state, particularly crystals of the antimoniates of magnesia, cobalt and nickel, which are isomorphous. They consist of regular six-sided prisms. In all the salts of antimonic acid which have been investigated, the proportion of the oxygen of the base to that of the acid is as 1 to 4·6 ; they therefore contain some excess of base which is united to water, so that these compounds are to be considered as salts which contain two acids, antimonic acid and water. The salts of antimonic acid also contain water of crystallization : in the crystallized salts this commonly amounts to twelve atoms, whilst if these salts are obtained in the amorphous state they usually contain only six atoms of water. The composition of the first may be best expressed by the formula

$$RO \; HO + 12(RO \; SbO^5 + 12HO).$$

If it be heated to 212° F. it loses 8 atoms of water; at 390° it loses 10 atoms, and at 572° 11 atoms. In the salts of antimonic acid heated to 572° we may therefore assume for one atom of antimonic acid two atoms of base, of which one is water ; and in the salts heated to 390°, three atoms of base, of which two consist of water.—*Berl. Monatsbericht,* 1852, p. 344.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1852.

Chiswick.—September 1. Slight rain. 2. Foggy : cloudless and hot. 3, 4. Very fine. 5. Overcast : clear. 6. Very fine : rain at night. 7. Hazy : rain. 8. Heavy rain : thunder and lightning : cloudy : clear at night. 9. Cloudy : showery. 10. Cloudy. 11, 12. Fine. 13. Very fine. 14. Clear : dry air : densely overcast at night. 15. Overcast. 16. Fine : clear and cold at night. 17. Slight fog, with very heavy dew : very fine : clear and cold. 18. Foggy : heavy rain. 19. Cloudy : uniformly overcast. 20. Slight rain : showery : clear. 21. Boisterous, with rain. 22. Clear : very fine. 23. Overcast : very fine. 24. Foggy : very fine : dense fog at night. 25. Dense fog : very fine. 26. Heavy dew : foggy : hazy throughout. 27. Dense fog : overcast : heavy rain at night. 28. Constant rain : barometer very low : foggy. 29. Cloudy : slight showers : cloudy. 30. Overcast : fine but windy : overcast.

Mean temperature of the month	56°·20
Mean temperature of Sept. 1851	55 ·15
Mean temperature of Sept. for the last twenty-six years ...	57 ·15
Average amount of rain in Sept.	2·52 inches.

Boston.—Sept. 1. Cloudy. 2. Fine. 3, 4. Cloudy. 5. Cloudy : rain A.M. 6. Cloudy : rain P.M. 7. Cloudy : rain : thunder and lightning early A.M. : rain P.M. 8. Fine. 9. Fine : rain P.M. 10. Fine. 11—13. Fine. 14. Fine. 15. Cloudy : rain A.M. and P.M. 16. Fine : rain early A.M. 17. Fine. 18. Fine : rain A.M. and P.M. 19, 20. Cloudy : rain early A.M. 21. Rain : rain A.M. 22. Fine. 23. Cloudy. 24. Fine. 25—27. Foggy. 28. Rain : rain A.M. and P.M. 29. Cloudy : rain A.M. 30. Fine.

Sandwick Manse, Orkney.—Sept. 1. Showers. 2. Clear : fine : cloudy : fine. 3. Clear : fine. 4. Bright : clear : fine : aurora. 5. Fog. 6. Hazy : cloudy. 7— 9. Fine : clear : fine. 10. Cloudy : showers. 11. Showers : drops : aurora. 12. Showers : aurora. 13, 14. Sleet-showers. 15, 16. Showers. 17. Bright : showers : aurora. 18. Drizzle : rain. 19. Bright : clear : rain. 20, 21. Bright : cloudy : rain. 22. Showers : cloudy. 23. Cloudy : rain. 24. Showers : clear. 25. Bright : cloudy. 26. Clear. 27. Showers : rain. 28. Showers : lunar rainbow. 29. Bright : cloudy : rain. 30. Rain : cloudy.

Mean temperature of Sept. for twenty-five years	52°·22
Mean temperature of this month	53 ·45
Average quantity of rain in Sept. for six years	2·49 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; at BOSTON, by Mr. Veall; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1852. Sept.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick Max.	Chiswick Min.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¾ p.m.	Boston 8½ a.m.	Boston 8½ a.m.	Chiswick Max.	Chiswick Min.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¾ p.m.	Chiswick 1 p.m.	Boston	Orkney, Sandwick	Chiswick	Boston	Orkney, Sandwick
1.	30·195	30·030	29·976	29·90	29·57	60	72	43	57½	58	sw.	sw.	s.	·01	·18	·18
2.	30·261	30·220	30·06	30·16	29·73	60	77	45	61	62	sw.	s.	se.			·07
3.	30·187	30·121	30·15	30·16	29·70	67	76	49	63	61	e.	se.	se.			
4.	30·093	30·024	30·15	30·16	29·60	66·5	75	51	63	60	e.	ese.	se.	·02	·10	
5.	29·990	29·935	30·15	30·05	29·48	66·5	73	50	63	60½	s.	n.	se.	·32	·57	
6. ☽	29·970	29·938	29·95	30·07	29·45	62·5	69	55	64	61	s.	nw.	ese.	·48	·37	
7.	29·929	29·918	30·23	30·49	29·39	65·5	69	55	67½	63½	n.	se.	calm	·14		
8.	29·982	29·951	30·38	30·41	29·53	65·5	69	58	63	60	w.	ne.	calm	·09	·07	
9.	29·926	29·859	30·13	30·31	29·53	65·5	69	59	60	52½	c.	ne.	e.	·10		·10
10.	29·907	29·822	30·13	29·91	29·42	67·5	69	49	58	50	ne.	ne.	nw.			·06
11.	29·862	29·826	29·83	29·83	29·33	58·5	67	45	50	46	ne.	nw.	n.			·16
12.	29·925	29·848	29·71	29·63	29·33	58·5	68	42	52	45	n.	nw.	nw.			·10
13. ●	29·937	29·896	29·77	29·77	29·41	53	67	46	48	44	e.	wnw.	nw.	·07		
14.	29·947	29·788	29·66	29·79	29·50	47½	63	45	47½	48	w.	wnw.	nw.	·11	·93	
15.	29·578	29·398	29·55	29·66	29·26	43½	63	34	48	48	nw.	w.	n.			·32
16.	29·817	29·682	29·69	29·64	29·47	52	61	37	53	52	s.	n.	wnw.	·84		·27
17.	29·849	29·811	29·76	29·78	29·30	56·5	62	43	50½	42	n.	calm	ssw.	·03	·87	·07
18.	29·643	29·226	29·57	29·65	28·92	57·5	66	45	55	45	c.	se.	nnw.	·23		·20
19.	29·639	29·263	29·45	29·46	29·10	48·5	63	34	44	56½	ne.	n.	wnw.	·01	·07	·03
20. ☽	29·593	29·531	29·57	29·57	29·11	56·5	65	47	50	55½	se.	s.	cne.			·01
21.	30·164	29·605	29·85	29·64	29·88	50	57	43	48	56½	w.	nnw.	w.			·03
22.	30·417	30·376	29·93	30·01	29·95	54	62	46	56	51½	v.	w.	w.	·01		·05
23.	30·462	30·457	30·06	30·02	29·98	53	66	43	58	44½	v.	w.	w.	·01		
24.	30·460	30·322	30·11	30·09	29·80	56	67	44	57	47	ne.	wnw.	sw.		·36	·23
25.	30·239	29·960	29·98	30·11	29·47	56	66	45	56	44	c.	sw.	wnw.	·63	·77	
26.	29·906	29·884	30·02	29·86	29·43	54	61	48	50	50½	e.	calm	nw.	·29	·03	
27.	29·869	29·692	29·95	30·07	29·04	50	61	48	58½	44	e.	calm	calm	·02		·07
28. ○	29·391	28·956	29·87	29·83	28·66		62	50	46½	50½	sw.	n.	mne.	·13		·25
29.	29·292	29·179	29·48	29·76	29·00		63	54	48	44	sw.	ssw.	mne.			·06
30.	29·674	29·470	29·33	29·31		50	59	42	47		sw.	w.	calm			·33
Mean.	29·936	29·799	29·881	29·886	29·41	57·0	66·20	46·20	54·76	52·15				3·54	4·32	2·64

LXIII. *On* Sir David Brewster's *New Analysis of Solar Light.*
By H. Helmholtz[*].

IN a series of papers[†] published by Sir David Brewster, he
has endeavoured to establish a peculiar view which he en-
tertains regarding the composition of solar light and the gene-
ration of colours. These papers must naturally attract in a high
degree the attention of physicists, both on account of the well-
won renown of their author in the domain of optical science,
and of the new facts which he adduces in support of his asser-
tions. According to him, solar light is compounded of three
different kinds of light, red, blue and yellow; and each descrip-
tion of light possesses rays of all degrees of refrangibility, but
so distributed that red light contains a preponderance of rays of
less refrangibility, yellow more rays of mean refrangibility, and
blue more of greater refrangibility; hence it is that the first pre-
dominates at the less refrangible end of the spectrum, the second
in the middle, and the third at the most refrangible end. The
remaining colours of the spectrum, orange, green, violet, are
supposed to be caused by the mixture of the three primitive
colours. The prism can only separate those rays from each other
which possess unequal degrees of refrangibility; if, however,
there exist different kinds of light of the same refrangibility, the

compound light formed by them must in the prismatic analysis behave as simple light.   To this Brewster replies, that such rays may be separated from each other by taking advantage of their difference of absorption in coloured media ; and he has attempted by means of this method to prove, that in all portions of the spectrum, rays of all three descriptions, and consequently the white light due to their union, is to be found.   The facts which he calls in to his support prove, he considers, that homogeneous light, in the sense of Newton, that is, light composed of rays of equal refrangibility (wave-length) only, sometimes suffers a change of colour in its passage through coloured media, while the universally accepted theory of Newton asserts that the colour of homogeneous light depends solely upon its refrangibility (wave-length) ; that such light may be weakened, nay, completely extinguished, in its passage through coloured media, but can never exhibit a change of colour.   We must certainly grant that, if a single case be established in which the colour of homogeneous light is changed by absorption in a coloured medium, Newton's theory must be abandoned, and that of Brewster, or one similar, must be assumed in its stead.

I remark in the first place here, that the number and nature of the three primitive colours assumed by Brewster are based upon indirect inferences.   In this respect he has retained the pretty generally received theory of the mixture of colours, according to which red, yellow, and blue are the components of all others ; yellow and blue, for example, producing green.   I have shown in another place* that this theory is based upon the results of the mixture of the coloured substances merely, but that the mixture of such substances is by no means equivalent to the mixing of lights of the same colours,—to cite a particular example, yellow and blue united do not produce green, but white. The three colours, red, yellow and blue, can therefore compose no green, and must, if we are to retain the idea of three primitive colours, be superseded by others, say red, green and violet, as already assumed by Thomas Young.   By this alteration Brewster's theory would undergo no essential change ; single conclusions only would require modification.   I will therefore not enter further upon this subject here, but limit myself to the investigation of the question, "Is the colour of homogeneous light altered by coloured media or not?"

Hitherto Airy†, Draper‡, and Melloni§ have sought to refute

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* "Ueber die Theorie der zusammengesetzten Farben," Müller's *Archiv für Anatomie und Physiologie*, 1852.

† Phil. Mag. vol. xxx. p. 73 ; Pogg. *Ann.* vol. lxxi. p. 393.

‡ Silliman's Journal, 1847, vol. iv. p. 388 ; Phil. Mag. vol. xxx. p. 345.

§ *Bibl. Univ. de Gen.* Août, 1847 ; Phil. Mag. vol. xxxii. p. 262.

the view of Brewster. The first dwelt particularly upon the fact that Brewster, in the method which he applied, had not the colours changed by absorption simultaneously with the unchanged colours before him, and could therefore readily make a mistake in the comparison. To this Brewster replied, and I can corroborate his statement by my own experience, that in his observations the changes of colour were for the most part sufficiently striking to be observed without difficulty. Draper and Melloni expressed their doubts regarding the purity of the spectrum used by Brewster, and thought that the single colours might overlap each other considerably. From Brewster's statements in reply to these attacks, it is evident that no such overlapping of the colours took place; his later experiments on the number of Fraunhofer's lines in the solar spectrum, show further that he possessed far more complete apparatus for the separation of the rays of different refrangibility than even Fraunhofer himself, or perhaps any other physicist. A careful repetition of at least the most important of his experiments, carried out in exact accordance with his method, and with every precaution hitherto deemed necessary, has indeed taught me that the facts which he affirms to have observed are described with perfect accuracy; indeed nothing else could be expected from so skilful an observer; I trust, however, to be able to show that his explanation of these experiments is untenable, and thus to remove the apparent contradiction of the views of Newton.

It is much to be regretted that Brewster nowhere gives a detailed description of his method of observation. Hence it is that Draper and Melloni might do him injustice by their suppositions, and hence also, that at the outset I must ask his indulgence in case I also should allude to possible sources of error which he has taken pains to avoid. Partly from his replies to his antagonists, and partly from the description in his Treatise on Optics, London, 1831, I gleaned the following regarding his mode of observation. In the shutter of a dark room he made a narrow aperture, and looked at this with the naked eye through a strongly refracting prism; in the spectrum thus formed he was able to detect the stronger lines of Fraunhofer. He then introduced the absorbing coloured medium between the eye and the prism, and observed the altered spectrum. Besides this, he repeated the experiment with spectra in which a number of dark bands were formed by interference, and the different colours separated in a still more evident manner. Brewster does not state whether it was direct sunlight, or merely the reflected light of the sky which he permitted to enter through the aperture and fall upon the prism. We must, however, assume that in most cases he made use of the former; for when the slit is sufficiently narrow

to show the lines of Fraunhofer, the coloured rays changed by the absorbing medium, and to which the experiments had reference, are for the most part so feebly luminous that they can be clearly seen by direct sunlight only.

The doubts which impressed themselves upon me during the repetition of these experiments, refer, in the first place, to the question whether small quantities of white dispersed light might not have been mixed up with the spectrum; and secondly, whether the eye under the given circumstances was not prevented by physiological influences from forming a correct judgement as to the colours. With regard to the doubt first expressed, it may be stated that by the method of Brewster all light, with the exception of that which enters through the aperture, may be completely cut off; with a good strongly refracting prism, or a combination of two such prisms and a narrow slit, it is possible, so far as the solar light is regularly refracted, to separate it very completely into its differently coloured rays, so that in the spectrum these shall not at all overlap each other. It must, however, be remembered that a small portion of light could obtain admission by another way than that of regular refraction. In the first place, the dispersion of the light at the limiting surfaces and in the mass of the glass merits consideration.

If a piece of glass, whether a prism or a lens, be directly shone upon by the sun and observed against a dark ground, no matter how clear or how highly polished it may be, a great number of shining points are always seen in its interior, and minute scratches upon its surface; both disperse irregularly a sensible quantity of light, and impart to the whole a smoky appearance. To render such an examination quite exact, let a sheaf of solar rays issuing through an orifice in a dark screen fall upon the piece of glass, and let the eye be brought nearly into the same line as the transmitted rays, so that the latter shall pass close to the eye but not enter it. The little irregularities of the surface and mass then appear brightly illuminated against the black ground furnished by the screen. The flint-glass prism cut by Plössl, which I made use of, and which with a telescope showed the lines of Fraunhofer in great number and perfection[*], was not free from such irregularities. Brewster himself has not left this point unnoticed; in his reply to Draper he observes, that besides the most beautiful prisms of glass, he also used prisms of rock-salt, of such homogeneity and purity that on looking through them the substance of the prism was invisible; but he does not say whether he tested them by direct sunlight in the manner I have described. In this way many imperfections are rendered

[*] It resolved, for example, the line D into its two component lines lying close beside each other.

visible which are totally imperceptible in ordinary daylight. Prisms of rock-salt were not in my possession, and I can therefore form no opinion as to their completeness.

The second circumstance to be taken into account is the repeated reflexion of light in the prism. In the majority of prisms used for experiments on dispersion the two refracting surfaces alone are polished, the other three being ground dull. If such a prism be placed upon a dark ground so that the dull surface shall be illuminated, then within the prism a series of reflected images of this surface is observed. The two polished sides act like an angular mirror, which exhibits a series of circular images of any object placed between its reflectors. In the case before us the third surface occupies this position, and we look through one of the reflectors into the interior. The reflected images of the third surface appear in exactly the same direction as the spectra which are observed on looking through the prism; and as a portion of the incident light usually falls upon the third surface, illuminating it and its images, a weak white luminosity is thus created which spreads itself over the spectrum. The quantity of the reflected light is certainly very small, and in general will not be at all observed beside the regularly refracted light. To cut it off, it is necessary to blacken all the surfaces well except the two refracting ones.

When the coloured media are introduced between the prism and the eye, it must be remembered that if the polish of their surfaces and the purity of their mass be not perfect, light will also be dispersed by them. As coloured media, Brewster used for the most part glass plates or coloured fluids, the latter of course enclosed between glass plates. Regarding the purity of the glasses I have just spoken; but in the case of fluids also, for example of distilled water, we know that through layers of a certain depth the light which passes is cloudy, that is, a portion of it is dispersed. Besides this, the reflexions which take place between two surfaces of the coloured medium, and between them and the cornea of the observer's eye, are also to be taken into account. When the coloured plate has parallel surfaces, the rays which have undergone repeated reflexion between them create secondary images of the spectrum which almost completely coincide with the original one, and cannot do much injury. If the surfaces are not parallel it would be more suspicious, for here the colours of the secondary images might fall upon other colours of the primary. To this it must be added, that the incident light is partly reflected by the cornea, and this reflexion again reflected by the glass plate; the image thus formed being too near the eye must appear as a bright luminosity in the field of view. On account of these circumstances, I prefer placing the

coloured media between the source of light and the aperture, to placing them between the prism and the eye. By this alteration a considerable quantity of dispersed light will be excluded from the field of view.

The description of all these circumstances may appear pedantic, and I am ready to admit that the irregularly refracted light must certainly form an extremely small portion of the light incident—a portion far too inconsiderable sensibly to affect the appearance of the spectrum under ordinary circumstances. It will, however, be seen that it is not too small when added to colours that have been already greatly weakened by absorption, to cause a sensible change in the tint of the latter.

The circumstances heretofore spoken of are such as possibly might be excluded in following out Brewster's method of experiment. Perhaps there are prisms which are able to withstand the foregoing test; Brewster's may perhaps have been properly blackened, and the coloured media placed before the aperture; then indeed regularly refracted light alone would reach the eye. But there are sources of error resident in the eye itself which cannot be avoided. I would invite attention to the fact, that when very bright light of any kind whatever falls upon a portion of the retina, light of the same kind appears diffused as a weak luminosity over a great portion of the field of view. The phænomenon is easy to be observed. Let a candle be placed in the evening in the neighbourhood of a large dark surface, for instance of a door which opens into a dark room, and let the degree of darkness of the surface be observed while the light is alternately concealed by the finger and allowed to strike the eye. It will be readily seen, that as often as the rays freely enter the eye a white luminosity appears spread over the surface, being brighter in the vicinity of the light, and spreading itself weakly over the more distant portions of the surface. The same is observed when daylight, and most strikingly when direct sunlight enters the eye from an orifice in a dark screen. When the orifice is covered by a coloured glass, the luminosity has the colour of the latter. I have observed this with my own eyes, which are in good condition, and have also shown it to many others. That the diffraction of the light by the eyelashes is not the cause of this is proved by the fact, that the phænomenon is observed when the lids are drawn far apart.

With regard to the cause of this phænomenon, it has been hitherto regarded by most observers who have noticed it as purely subjective; it was believed to be referrible to an extension of the excitement to the adjacent fibres of the retina. But it can be shown that circumstances exist which must cause a small portion of objective light dispersed within the eye to reach other portions

of the retina than those affected by the regularly refracted light. To these belong undoubtedly the diffraction of light in the pupil. When light passes through a narrow orifice, or simply passes the edge of a dark body, a small portion of it will be always deflected. Now although the pupil is certainly too large in comparison to the focal distance of the eye to permit of rings being formed and a considerable portion of light dispersed, as is the case when a very small aperture is held close to the eye, still the diffraction is by no means completely annulled. Further, it may be regarded as questionable whether the media of the eye are absolutely clear; being partly composed of microscopic cells and fibres, as the cornea and crystalline lens, and in other places traversed by a great number of fine membranes, as in the vitreous humour. The presence of little irregularities in the structure of the backward portion of the vitreous humour is further indicated by the so-called midges of the field of view, and perhaps something similar is to be found in other portions of the eye. By these also light must be dispersed. Finally, it is proved by the eye-mirror constructed by me*, that a tolerable quantity of light is sent from the illuminated portions of the retina to the pupil, and this must be reflected back again from the forward surface of the cornea. It is therefore to my mind an undoubted fact, that a portion of the light incident upon the eye is deflected so as to fall upon other portions of the retina. Whether along with this an extension of the nervous excitation over the retina takes place, cannot be decided without further investigation; for our purpose it is, however, a matter of indifference whether objective light, or only its subjective perception, is diffused over the retina.

I will now attempt to prove that one of Brewster's most striking results is derived from a mixing of the regularly refracted light with other light which has been dispersed partly without and partly within the eye. I allude to the isolation of white light in the yellow of the spectrum by glass coloured blue with smalt. It is known that by such glass dark bands are generated in the less refracted portion of the spectrum. Between them stand several coloured bands, namely (1) the extreme red, embracing the lines A and B, quite unaltered; (2) a band of reddish-orange between the lines C and D, extremely weak; (3) a yellow band, at one end verging into orange, at the other end into green, less weakened than the foregoing. Between this yellow and the green occurs an interval not totally dark, while blue and violet are transmitted without diminution. Brewster draws attention to the fact, that while the primitive colour of the yellow band was a rich gamboge, the same viewed through a certain thickness

* See my description of an eye-mirror (Augenspiegel) for the investigation of the retina of living eyes. Berlin, 1851.

of the glass appeared a dull yellow; through a still greater thickness it appeared to be a greenish-white; and on introducing other colouring matters, particularly solutions of copper and red ink, it finally changed into white. This white he further asserts is not to be decomposed by the prism; but if I rightly comprehend the meaning of his expressions, he has never tried this by a second prism—which indeed could not be effected without a considerable modification of the method of experiment—but infers it merely from the fact that this white light has passed undecomposed through the first prism.

The blue glass which I had at my disposal showed the phænomena described by Brewster in the following manner. Seen through one plate, the yellow stripe in a spectrum produced by the light of the firmament was very feebly luminous, and a greenish-yellow; but in the spectrum obtained from the portion of the heavens which lay near the sun it was a pure and shining yellow. Observed through two plates, the stripe obtained from daylight disappeared totally; with direct sunlight it appeared almost white; with greater intensity of light it verged into greenish-yellow, and with diminished intensity into blue. Adjacent to this moderately illuminated band the blue and violet appeared of course splendidly bright, and the extreme red was also strongly luminous. Seen through three plates in direct sunlight, the yellow band appeared a bluish-white. The alteration of the colours was somewhat less when the plates, instead of being introduced between the prism and the eye, were placed before the aperture, that is, between the source of light and the aperture. When we consider that the sun is upwards of 50,000 times brighter than the brightest white surface which he illuminates, and that the yellow in the original spectrum possesses the intolerable brightness of the sun, but seen through two blue glass plates appears as a moderately illuminated surface of paper, in the absence of more exact measurements we shall not be far from the truth in assuming that the hundredth part of the yellow passes through one glass, and the ten-thousandth through two. Now supposing that only the ten-thousandth part of the coloured light which passes unweakened through the plates is caused, by the little irregularities before spoken of, to fall upon the same portion of the retina as that which receives the yellow, we must certainly obtain colours very different from the latter. By the mixture of indigo-blue light with yellow, we obtain, as I have shown in my investigation on compound colours, first a whitish yellow, then white, which finally passes into a bluish-white. The colours in the smalt-glass spectrum which lie next to the yellow, namely red and green, can, by mixing in various proportions, cause the white to approximate to red or green, as

the case may be, and thus exhibit all the degrees of colour which are observed through different thicknesses of the blue glass.

According to the method of Brewster, all parts of the spectrum, weakened and unweakened, are before the eye of the observer at once, and it is therefore impossible to prevent the irregularly dispersed portion of the brighter colours from entering the eye. Hence the problem reduces itself to the finding out of another method of repeating these experiments, by which the disturbing colours shall be totally, or almost totally, excluded from the field of view. If the spectrum be observed through a telescope, it is possible to procure any desired colour isolated from the others, but the irregular refraction and reflexion of the light without the eye will be increased by the glasses of the telescope. The changes of colour of the yellow stripe I found certainly less when they were viewed thus singly, but nevertheless they still existed. Another method, however, gave me perfectly satisfactory results. The method is derived immediately from that of Brewster, if instead of permitting the unchanged sunlight to enter through the aperture, we transmit it first through a prism, and then permit those portions only to pass through the aperture whose changes of colour are to be investigated. My manner of proceeding is as follows :—Solar rays reflected from a mirror are permitted to enter through a narrow slit into a dark room and to fall upon a vertical prism. Immediately behind the latter is a lens which casts the spectrum formed by the prism upon a screen. In the latter is a second very fine vertical slit. The light of that band of the spectrum which falls exactly upon the slit passes through, while the rest is cut off. The observer stands behind this second screen, the back of which is well blackened, best covered with black velvet, and looks at the slit through a second prism of the best possible quality. If in the first prism, or in the lens, no light was dispersed, then would homogeneous light alone of a determinate colour arrive at the second slit and pass through it; and this, on account of its homogeneity, would, when looked at through the second prism, form no spectrum, but remain a narrow band, just as if it were seen by the naked eye. But as a small portion of white irregularly-refracted light enters, a very feebly luminous spectrum is formed by the latter, in which a single coloured band, that of the regularly refracted light, comes very brilliantly forward. The light dispersed in the second prism and in the eye, belonging, as it does, for the most part to that of the bright band, cannot when mixed with the latter change its colour, for it is homogeneous with it. Of the other colours, those only which are irregularly refracted in the first prism pass through the slit; and this quantity of light is so small, that the portion of it dispersed in the second prism and in the eye of the observer cannot be further perceived.

By this method we can obtain bright a band of any breadth whatever, if instead of the first slit a rectangular opening of greater or less width is cut out. The spectrum of the first prism will then be an impure one; that is, at every point of it the neighbouring bands of colour will overlap each other to a certain extent; hence regularly refracted light of different kinds passes through the slit and is decomposed by the second prism into its component tones of colour. In this way is obtained a more or less bright, sharply-defined band, furnished with its appropriate lines of Fraunhofer, and composed of those overlapping colours which fell from the first prism upon the slit; while the remaining portion of the second spectrum, illuminated merely by the dispersed light, remains very feebly luminous. In this way the violet at the other side of the line H, which when the other colours are present is usually regarded as invisible, can be rendered surprisingly distinct, being obtained free from white light for a width equal to that between the lines G and H. When viewed in the ordinary way through a telescope, the remaining portion of the spectrum being shut out, it is usually mixed with an inordinate quantity of white light.

If we isolate the light of the yellow band of the smalt-glass spectrum according to this method, and subject it to the absorption of a certain number of plates of the glass introduced before the first or the second slit, or before the eye, we obtain results totally different from those arrived at by the method of Brewster. The yellow retains its originally pure and saturated colour after it has passed through two, three, or even four plates of the blue glass. I may further remark, that an absolutely dark room is not essential to the success of this experiment, if care be taken that the second screen is sufficiently black, and the plates of glass are placed before the first slit.

Brewster's explanation is irreconcileable with this observation. According to his view, the light of the yellow band, when rendered whitish by the cobalt-glass, is composed of rays of equal refrangibility, and hence by refraction in prisms cannot be further decomposed into rays of different colours. In the experiment above described, the light of the yellow band on entering the first slit appears actually whitish; but when it is viewed through a second prism, it is decomposed into pure yellow and light of other colours; hence it does not possess the same refrangibility, but, in accordance with the explanation given by me, is a mixture of rays of different refrangibilities. In Brewster's proceeding, a mixture of foreign light, whether in the prism, glass plates or eye, could not be avoided. From this point of view it is quite intelligible how the introduction of coloured media might render the white colour of the band in question more pure, or cause it to approximate to red or green.

A second possible source of error is to be found in the physiological effect of contrast, which might easily prejudice the judgement of the colours, particularly when we observe a weakly illuminated space beside one which is brightly illuminated. Brücke* has lately drawn attention to the fact, that even quite obscure portions of the field of view appear, beside bright colours, to have a luminosity poured over them; that this luminosity is sometimes of the same colour as the light which excites it, sometimes complementary to the latter, and sometimes altogether different. He names the colour of this luminosity the induced colour. By the degree of brightness which he made use of, he found that red induced its complementary green, but that green induced green, violet, blue, but that blue and yellow did not induce any decided colour. A repetition of these experiments with different degrees of brightness, convinces me that the expression of Brücke must be modified; when very bright light is made use of, the same colour is always shed over the dark portion of the field, a phænomenon the possible cause of which has been spoken of above. With weak light the induced colour is always the complementary one, which, as Brücke also has remarked, becomes much more vivid when the eye is moved than when it is fixed upon a point; with medium light the deportment of different colours is different; sometimes the same colour, sometimes its opposite is produced; sometimes indefinite colourings, as if the opposed phænomena were struggling for pre-eminence. I have also found, in coincidence with the observation of Brücke, that the complementary colour of red is more easily induced than those of violet or green.

To this source it appears to me must be referred a surprising experiment of Brewster's, by which he sought to demonstrate the presence of green light in yellow, orange, and even in red towards the line C. As absorbing medium he made use of port wine, Peruvian balsam, pitch, sulphur-balsam, or red mica. I have repeated the experiments with Peruvian balsam, sulphur-balsam and pitch. Thin layers of these substances permit the red, yellow, and green of the spectrum to stand, while they extinguish blue and violet. In this case, however, green appears to extend as far as the line D, whose real position is in golden-yellow, and frequently reaches even beyond this to the vicinity of the reddish-orange. The green seems to abut immediately against the red. Hence the yellow-green, yellow, golden-yellow, and even the orange tone appear to have become green, and the latter is so vivid that it is indeed difficult to conceive that it could be a subjective illusion. The presence of such an illusion

* " Untersuchung über Subjective Farben," *Denksch. der Akad. d. Wissenschaft zu Wien,* vol. iii.

is, however, indicated by the circumstance, that the limit of the green extends much further when the eye is permitted to wander over the different colours of the spectrum than when it is persistently fixed upon the green portion. In the first case, the yellow colours strike the retina on places which before were acted upon by the shining red, and therefore tend to generate the complementary blue-green; in the second case, the excitation of the subjective colour upon the contiguous portions of the retina is much feebler. That the phænomenon is due to a subjective illusion is immediately shown when the colours are isolated according to the method which I have above described, and then looked at through layers of the above-named brown bodies of different thicknesses; they then appear totally unchanged, and without the slightest tendency to green.

Looked at through thicker layers of the brown fluids, green, yellow, and a portion of the orange disappear from the spectrum. At the edge of the red which remains, a weak rim of green is observed, even near the line C, where the red has scarcely the appearance of orange. The green rim is too weakly luminous and narrow to permit its light being isolated and singly examined. That, however, weak orange light beside strong red may appear green, is easily shown by sticking a small disc of paper coloured red by vermilion upon a plate of red glass, and holding the latter against a very bright ground, the bright firmament for example, while the disc is only weakly illuminated. With a suitable strength of illumination it appears green*.

It further appears to me, that the violet colouring of the blue to the vicinity of the line F, through absorption by yellow fluids, olive oil, sap of the *Coreopsis tinctoria*, &c., belongs to these subjective complementary phænomena. I have repeated the experiment with olive oil, and have plainly seen the violet between the lines F and G nearly as far as F, but only when this portion of the spectrum was very feebly illuminated.

The oil does not sensibly change the brightness of the red, yellow and green; it weakens the blue considerably, and almost extinguishes the violet. When I permitted the light of bright clouds to enter through the slit, the first-named colours appeared bright, the blue feeble, and the violet was not at all to be seen. When, however, direct sunlight was passed through the slit, the portion between the lines F and G appeared brighter and lost its violet appearance. When isolated from the other colours of the spectrum in the manner before indicated, the blue appears in its

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* The subjective colour is very strikingly exhibited in this experiment. A red wafer answers the purpose perfectly; the observer stands in a weakly illuminated place and looks at the sky, the wafer appears a vivid green or blue-green.—J. T.

true colour. I believe, therefore, that in the spectrum observed through olive oil, the carmine-red complementary to the bright green which lies contiguous to the blue is shed over the latter, which is thus rendered violet.

There are also other methods which I can recommend in this and similar cases of testing. Let the absorbing substance be placed before the greatest portion of the slit, and before the remaining portion white paper, thick or thin, oiled or not oiled, which is so chosen that the place to be investigated in the absorption spectrum shall be equally bright with the corresponding place of that formed by the light which has passed through the paper. When the absorption is by oil, it will be seen that in the paper spectrum also the blue between the lines F and G appears violet. For the experiment to succeed, it is necessary that the breadth of the absorption spectrum shall be much greater than that of the paper spectrum.

By these facts it is plainly proved that subjective changes of colour can take place in the spectrum, not only in the same degree as when ordinary colours are brought together, but perhaps more striking and illusive, on account of the greater vividness of the simple colours. In other cases these changes cannot be referred to the induction of complementary colours. An example of this, to which Brewster refers, is the band in reddish-orange, which extends about from C to D in the smalt-glass spectrum. It is much darker than the red and yellow portions which lie next it, and seems when looked at between these with an ordinary brightness of spectrum, to possess exactly the same red tone as the extreme red. In a more brightly illuminated spectrum it is observed to pass into orange. Brewster first called the band orange-red*, but afterwards† affirms that Sir John Herschel found it to be pure red ‡, and thought he had observed a change wrought in it by absorption. In this case also the separation of the band from the remaining portion of the spectrum proves that its colour is not in the least degree changed. The same remark applies to the green-blue tones of colour on the green side of the line F, which, as Brewster remarks, on being looked at through a deep blue glass (probably the smalt-glass) become green. When they are isolated and examined there is no alteration of colour observed.

Finally, in some of Brewster's experiments another physiological circumstance comes into play; the same homogeneous light at different degrees of intensity does not excite the same impression of colour. When dazzlingly bright, all colours appear white.

* Edinburgh Transactions, vol. ix. part 2. p. 439.

† In his reply to Airy.

‡ Treatise on Light, art. 496 and 506.

This is most easily observed with violet, which, in the spectrum of direct sunlight and by a moderate degree of brightness, appears to be a white-gray, retaining only a feeble tinge of violet.   Professor Moser has shown me that the sun seen through a dark violet glass appears as completely white as the strongly illuminated clouds observed on looking past the glass.   In like manner blue, of a degree of brightness which may be borne without injury to the eye, appears whitish-blue, and, if the brightness be increased, appears white.   Green first becomes yellow-green before, like yellow, it entirely loses its colour with increased brightness.   Red exhibits the phænomenon with more difficulty than all others; and only by the highest degree of brightness have I been able to see it bright yellow in the spectrum, or the sun of the same colour when looked at through a red glass. While experimenting on this subject, in order to prevent the admixture of all light of other colours, I have made use of the coloured bands of the solar spectrum which were separated and purified by two prisms in the manner already described.   Different degrees of brightness were obtained by applying the light of brightly illuminated portions of the firmament; but as, according to Brewster's theory, the colours of the spectrum produced by sunlight are not the same as those produced by the light of the firmament, I also made use of the direct solar rays, sometimes observing them directly, sometimes transmitting them through two Nichol's prisms crossed perpendicularly.   By reflexion from uncoated glass plates, or by receiving them upon a white screen, the brightness can be dimmed without fear of altering any colour.

If, therefore, a certain thickness of the solution of ammonio-sulphate of copper shows the blue of the spectrum bright and whitish, while a greater thickness causes it to appear a deep dark blue, we are simply to conclude that this fluid absorbs blue rays, but by no means that it has abstracted white light from the blue.   Herein also we find the explanation of the fact, that the yellow in the spectrum of daylight or of the blue firmament is scarcely discernible, while in the spectrum of direct sunlight it takes up a wide space.   The pure yellow forms in a flint-glass spectrum an extremely narrow band, and with the blue light of the firmament is weaker than its neighbouring colours, so that in the slightly magnified spectrum it is hardly discernible between the wide and shining red and green.   When considerably magnified, on the contrary, or when viewed isolated, it is very plain even with daylight.   In the spectrum of direct sunlight, however, yellow is the most prominent colour and of dazzling brilliancy.   Green and red, with increased intensity, become also yellowish, and hence it is that yellow appears so prominent.   If,

however, the solar spectrum be enfeebled by reflexion from un-coated glass plates, or by a pair of Nichol's prisms crossed nearly at right angles, the yellow recedes and appears as in the daylight spectrum. If, besides this, the colours belonging to various groups of Fraunhofer's lines be isolated and determined in a spectrum of moderately strong sunlight and in one of daylight, they will be found quite alike.

Of the facts which Brewster adduces in support of his theory one remains over, with regard to which I do not know whether I can say that I have succeeded in repeating it, and some others which I was unable to repeat, not having the proper absorbing media in my possession. The first is obtained with Peruvian balsam, sulphur-balsam, pitch and mica. The red of the spectrum seen through these media is said to appear orange. With a moderate intensity of light I could observe nothing of the kind, no matter what might be the degree of thickness of the pitch or the balsam; the red retained its colour quite unchanged. Only with light of a greater intensity, and when a brown luminosity which surrounded the place gave evidence that a considerable quantity of light was dispersed, did I see the red somewhat orange. This, however, in the case under consideration, is to be referred to the admixture of the dispersed brown of the light compounded of red, yellow, and a little green, and to the tendency of red to appear yellow when the light is intense. Perhaps Brewster also made use of such a bright spectrum. When the red is isolated according to my method, its colour remains totally unchanged.

Various experiments have been made by Brewster with transparent wafers formed of gelatine. I could not obtain such here; and as their colours alone, and not the colouring matters, are mentioned, I was unable to make them myself. They seem, however, to me to be not free from objection, at least when placed between the prism and the eye, inasmuch as the best glutinous plates when formed of the purest isinglass between plates of glass, do not belong to the class of transparent bodies. Even when we are able to see pretty clearly through a single one, several placed one over the other make the image cloudy—a proof that they disperse a considerable quantity of light. This would indeed explain the action said to be exhibited by orange, yellow, and green wafers—the turning of the red of the spectrum orange. The dispersion of the predominant coloured light over the red is sufficient for this. How a green wafer generates a white band in the blue I am unable to say, inasmuch as I cannot repeat the experiment.

A pale red glass which absorbs the green between *b* and F (probably coloured with purple of Cassius) and a pale yellow

which weakens the blue, are said when combined to convert the blue into violet. The explanation is the same as for olive oil.

Red, reflected from a plate of brass, becomes orange according to Sir John Herschel. The means of explaining this is given by Airy in his memoir against Brewster.

I have now mentioned all the facts adduced by Brewster. Although I have been unable to repeat all his experiments, I believe the discussion of those which I have succeeded in repeating, abundantly proves that in his method many hitherto unobserved influences come into play, which render a sure judgement of the colours impossible and deprive his arguments of all force. If the assumed connexion of the refrangibility or length of wave with colour is to be proved erroneous, it must be done by some more certain method of observation, similar, for example, to that which I have described in this memoir; a principal condition of which is, that the colour investigated be separated from the other colours and rendered free from every trace of irregularly dispersed light.

LXIV. *On the Colours of a Jet of Steam and of the Atmosphere. By* R. Clausius.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the August Number of the Philosophical Magazine (p. 128) Mr. Reuben Phillips describes a series of interesting experiments on the colours of a jet of steam, which connect themselves with the known experiments of Prof. Forbes upon the same subject. At the end of his paper Mr. Phillips writes,—

"Prof. Forbes, after discovering the red colour of a jet of steam by transmitted light, connected the red colour of the clouds with this fact; and the truth of this connexion is beyond dispute. So far, however, as I have been able to go, the colours of the steam-jet are manifestly only influences of ordinary interference, greatly resembling that produced by thin transparent plates. Thus in (192) the transmitted light is red, as in Prof. Forbes's experiments, but the reflected light is blue. It is therefore to be inferred, that all the colours of the clouds originate in interference, caused by minute drops of water, the size of which determines their colour; while the blue jet (192) is, I think, strictly analogous to the blue sky."

With reference to this passage I permit myself to make the following remarks :—The blue colour of the firmament and the morning and evening red were explained by me in 1849[*] upon

the principles of 'ordinary interference;' and some time after-wards* I applied the same explanation to the colours of a jet of steam observed by Prof. Forbes.

In one point, however, my view diverges from that of Mr. Reuben Phillips. He names the water-particles which cause the interference "*drops* of water," while I believe that they are water-*bladders*, for which view I have adduced my reasons in a separate paper†.

Besides this, I should like to mention two points with regard to which I have been unable to obtain from the paper of Mr. Phillips a clear notion of the author's opinion.

(1.) Among the various colours of the atmosphere there appears to me to exist only two simple originating ones; namely, the *blue* colour in all its shades, from dark blue to white, due to interference by *reflexion*; and *orange-red* colour in the corresponding shades, due to interference by *transmission*. The other colours exhibited at times in various portions of the heavens, as, for example, purple or green, I hold to be due to the mixing of the above two colours in their different shades.

(2.) When clouds appear coloured, I believe that the colour exhibited is for the most part not formed in the cloud itself, inasmuch as the little bladders generally differ too much in thickness to cause the production of a single determinate colour; but that the light, partly on its way to the cloud, and partly between the cloud and our eye, assumes its colour; even in the apparently clear air there always exist bladders, which, however, are for the most part so attenuated, that they favour in a particular manner the formation of the *first* colours of interference, namely, blue and orange-red.

<div style="text-align:center">I remain, Gentlemen,
Very respectfully yours,</div>

Berlin, Oct. 13, 1852.   R. CLAUSIUS.

LXV. *On some Demonstrations in Geometry.* By JOHN POPE HENNESSY‡.

WERE it possible to give direct demonstration for every proposition in Euclid's Elements of Geometry, it would add, if not to the strength, at least to the beauty of that celebrated chain of reasoning; for the *reductio ad absurdum*, as the

* *Die Licht Erscheinungen der Atmosphäre*, described and explained by R. Clausius. Leipzig, E. B. Schwickert, 1850. Also under the title *Beiträge zur Meteorologische Optik*, published by John Aug. Grunert. Part 1. No. 4. p. 395; and in Pogg. *Ann.* vol. lxxxiv. p. 449.

† Pogg. *Ann.* vol. lxxvi. p. 161.

‡ Communicated by the Author.

most eminent editor of Euclid remarks, "only proves that a thing *must* be so, but fails in showing *why* it must be so; whereas direct proof not only shows that the thing *is* so, but *why* it is so."

To render the first book of the Elements in this respect perfect, it would be necessary to alter the proof of ten propositions; viz. the VI., VIII., XIV., XIX., XXV., XXVI., XXVII., XXIX., XXXIX. and XL.   Dr. Lardner has given direct proofs to the VIII. and XXV., and I have done so to the XL.*, leaving seven still proved indirectly; of these I will proceed to show how the VI., XIX., and XXVI. may receive direct demonstrations; of the remaining four I believe the XXXIX. will at some other period be proved directly, but that the XIV., XXVII. and XXIX. never will.   I am led to form this opinion of the three last because they rest on defective premises; the XIV., on the definition of a right line, which is unintelligible; and the XXVII. and XXIX. on the theory of parallels.

According to all geometricians, the fundamental rule in geometry is, "that the truth of a proposed principle is to be deduced from the axioms and definitions or other truths previously and independently established."   We may therefore place the VI., XIX., XX., XXI. and XXVI. after the XXXII., because that proposition and all others before it are proved independently of these five.

With this arrangement, the following direct demonstrations of the VI., XIX. and XXVI. can be given :—

VI. "If two angles (B and C) of a triangle (BAC) be equal, the sides (AC and AB) opposed to them are also equal."

Find a point D which is equidistant from the three vertical points of the triangle, (X., XII. and IV.).

The angles BCD and CBD are equal (V.), therefore DCA and DBA are equal; but these are respectively equal to CAD and BAD, therefore CAD and BAD are equal; therefore the remaining angles ADC and ADB are equal (XXXII.).   In these two triangles the sides AD and DC are equal to AD and DB, and the included angles equal, therefore AC and AB are equal (IV.).

XIX. If in any triangle (BAC) one angle (B) be greater than another (C), the side (AC) opposite the greater angle is greater than the side (AB) opposite the less.

From the point C draw CD parallel to AB and equal to AC.

As the angle ABC is greater than BCA, the angle DCB which is equal to ABC (XXIX.) must be greater than BCA, therefore the line
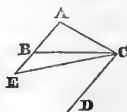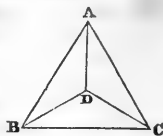
bisecting the angle ACD must pass between BC and CD. Draw this line, and produce AB to meet it.

As AE and CD are parallel, DCE and AEC are equal (XXIX.), therefore ACE and AEC are equal, therefore AE and AC are equal (VI.), and therefore AC is greater than AB.

XXVI. If two triangles (BAC, DEF) have two angles, and a side similarly placed with regard to the equal angles, equal, these triangles are equal in every respect.

Place them so that the bases form one right line, and the equal sides AC and DE are next each other. Produce BA and FE until they meet at G. Join A and E. As the angles ECF and CEF are equal to ACB and CAB, the remaining angles ABC and EFC are equal (XXXII.). Therefore BG and FG are equal (VI.). As AC and CE are equal (Hyp.), the angles DEA and CAE are equal, therefore GAE and GEA are equal, and therefore GA and GE are equal; taking these from BG and FG, we have AB and EF equal, and therefore (IV.) the triangles are equal in every respect.
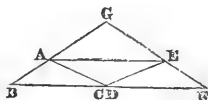
If the angles ABC and EFC are obtuse, the point G will lie at the other side of BF, but the proof will remain the same.

If, however, ABC and EFC are right angles, a different demonstration must be adopted.

Produce AC and EC until the produced parts are each equal to AC or EC. Join BD and FG. In the triangles ACB and DCB, the sides AC and CB are equal to DC and CB and the included angles equal, therefore AD forms one continued right line. In a similar manner EG is proved to be one right line. In the triangles ACD and ECG, AC and CD are equal to EC and CG, and the included angles (XV.) are equal, therefore AD and EG are equal, and therefore their halves AB and EF are equal.
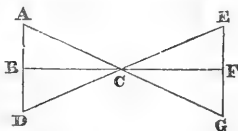
Queen's College, Cork.
    Sept. 28, 1852.

LXVI. *On the Reduction of Temperatures by Electricity.*
    By Dr. JOHN TYNDALL, *F.R.S.*

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN an abstract of Professor William Thomson's Mechanical Theory of Thermo-electric Currents, given in your Supplementary Number for July, reference is made to the well-known

experiment of Peltier on the absorption of heat at a bismuth and antimony joint. This has drawn from Mr. Adie a brief communication, published in your Number for September, from which it appears that the writer has never been able to obtain Peltier's result; he virtually denies its existence, and affirms the true state of the case to be that *less* heat is developed at some junctions than at others, but that *cold* is never generated. An objection precisely similar to that now urged by Mr. Adie induced Lenz to repeat the experiment fifteen years ago[*]. To the experiment of Lenz I took the liberty of drawing Mr. Adie's attention in your October Number; I did so because Mr. Adie had never mentioned it in his remarks, and it seemed to me to offer a proof of the absorption of heat so obvious as to be immediately appreciated. It does not however appear so to Mr Adie, for in your last Number I find that he suggests a hygrometric action as the probable cause of the diminution of temperature observed by Lènz. I should ill occupy your space were I to dwell upon conjectures where the 'law and testimony' of experiment are so near at hand, and fact so readily attainable. If the following results do not convince Mr. Adie, they will perhaps be the means of clearing away whatever doubt his remarks may have created in the minds of others.

Experiment No. 1.—In Plate IV. fig. 1, A is a bar of antimony, B a bar of bismuth, both bars being brought into close contact at J. To the free ends of the bars the wires $w\ w'$ are soldered, and dip into the little pools of mercury $m\ m'$; c is a piece of cork through which the wires pass, and by taking which in the fingers the wires $w\ w'$ may be easily moved from the pools $m\,m'$ to $m\ m''$, the warming of the wires being prevented by the cork. From $m\ m''$ wires proceed to a galvanometer, G, whose needles prove themselves to be perfectly astatic by setting at right angles to the magnetic meridian[†]. B is a single cell of Bunsen, from which, when matters stand as in the figure, a current can be sent through the bismuth and antimony pair.

The voltaic circuit having been established, the current—a very feeble one—was permitted to circulate for two minutes, its direction being from antimony to bismuth across the junction; at the end of the time specified the wires $w\ w'$ were moved from $m\ m'$ to $m\ m''$, a thermo-circuit being thus formed in which the galvanometer was included; the index of the instrument was at once deflected, and the extreme limit of its first impulse was noted; it amounted to

<div align="center">75°.</div>

[*] Poggendorff's *Annalen*, vol. xliv. p. 342.
[†] For an explanation of this, see an abstract of Du Bois Reymond's Researches on Animal Electricity, edited by Dr. Bence Jones.

The deflection in this case was similar in direction to that produced when the warm finger was placed upon the junction.

The wires $w w'$ were moved back to their former position, and the apparatus was suffered to cool; by crossing the wires $b b'$, causing the former to dip into m and the latter into m', the voltaic current was reversed, its direction across the junction being now from bismuth to antimony; the same time of circulation being allowed, on establishing the thermo-circuit, as before, a deflection of

$$68°$$

was observed. The deflection was the same as that produced when a small glass containing a *freezing mixture* was placed upon the junction.

But Mr. Adie will probably urge, that it is not the cold developed at J, but the heat developed at some of the other points, which caused the deflection here. I will not pause to discuss the objection, but will proceed to an experiment which deprives it of all force.

Experiment No. 2.—AA' is a bar of antimony, BB' is a bar of bismuth cast as in fig. 2, and in contact at the centre. From the cell B a current was sent through the system, and during its circulation the ends $g g'$ were unconnected; neither heating nor cooling of these ends by the current was therefore possible. The direction of the current across the junction was first from antimony to bismuth. After a short period of circulation the current was interrupted, and the ends of the wires $w w'$ were dipped into the mercury cups $g g'$, which were also in contact with A'B'; the index was driven through an arc of

$$40°.$$

The sense of the deflection in this case showed that the junction had been *heated*.

The current was reversed, its direction across the junction being now from bismuth to antimony; proceeding as before, the deflection was

$$30°.$$

The sense of this deflection was the same as that produced when the temperature of the junction was *lowered* by a freezing mixture.

I see no escape here from the conclusion that heat has been absorbed; for the ends $g g'$, exposed as they are to the atmosphere, must have its temperature, while the ends $m m'$, on which suspicion might reasonably rest, the current having passed through them, are wholly excluded from the thermo-circuit. The reader will observe that this is merely a modification of Lenz's experiment with the metallic cross.

But Mr. Adie has tried the cross, and it does not satisfy him;

very well, we will discard it, and proceed at once to an *experimentum crucis*. If the arms A′ B′ are not actually included in the voltaic circuit, they may seem to be in suspicious connexion with it. We must remove this source of doubt.

Experiment No. 3.—A and B, fig. 3, represent, as before, the bismuth and antimony couple, united at one end. M is a small chamber, hollowed out in a piece of cork and filled with mercury. A′ B′ is a second delicate thermo-electric pair, connected with the galvanometer, but wholly unconnected with A B. The wires w $w′$ are sufficiently strong to support A′ B′, so that the junction stands vertically over M, a slight pressure being sufficient to cause the wedge-shaped end of the pair to descend into the chamber of mercury. The whole arrangement was permitted to remain in a room until the temperature of the surrounding atmosphere was attained. Matters being in this state, when the pair A′ B′, which I will call the *test-pair*, was dipped into the mercury M, no effect was produced on the galvanometer. Now the mercury must partake of the changes of temperature of the junction with which it is in contact, and the nature of these changes will be ascertained with great precision by examining the mercury at proper intervals by means of the test-pair.

The voltaic circuit was closed, and the current allowed to circulate for three minutes, passing in the first place from bismuth to antimony. The current was then interrupted, and the test-pair was immediately dipped into the pool of mercury; the index of the galvanometer was driven through an arc of

$$40°.$$

The deflection was similar to that produced by immersing the end of the test-pair in a freezing mixture. Hence in this case heat was undoubtedly *abstracted from the mercury* during the passage of the current.

The apparatus being permitted to resume its equilibrium, the voltaic current was caused to traverse AB in an opposite direction. At the end of three minutes the test-pair was again immersed, and a deflection of

$$45°$$

was the consequence. The deflection was opposed to the former one, and demonstrated the *generation of heat* at the junction.

I am at present unable to see what possible objection can be brought against this last experiment. A hygrometric effect is out of the question; and the test-pair A′B′ being wholly unconnected with the voltaic current, cannot in any way be influenced by the latter. The results observed are evidently pure effects of the heating and cooling of the junction.

It will perhaps be permitted me to cite a single additional experiment, which exhibits all the necessary evidence without the reversion of the voltaic current.

Experiment No. 4.—B, fig. 4, is a curved bar of bismuth, with each end of which a bar of antimony, A, is brought into close contact. In front of the two junctions are chambers, hollowed out in cork and filled with mercury as before. A current was sent from the cell B in the direction indicated by the arrow; at M it passed from antimony to bismuth, and at M' from bismuth to antimony. Now if Peltier's observation be correct, we ought to have the mercury at M warmed, and that at M' cooled by the passage of the current. After three minutes' circulation the voltaic circuit was broken, and the test-pair dipped into M'; the consequent deflection was

<div align="center">

38°,

</div>

and the sense of the deflection proved that at M' heat had been *absorbed.*

The needles were brought quickly to rest at zero, and the test-pair was dipped into M; the consequent deflection was

<div align="center">

60°;

</div>

the sense of the deflection proved that at M heat had been *generated.*

The system of bars represented in fig. 4, being imbedded in wood, the junction at M cooled slowly, and would have taken a quarter of an hour at least to assume the temperature of the atmosphere. The voltaic current was reversed, and three minutes' action not only absorbed all the heat at M, but generated cold sufficient to drive the needle through an arc of 20° on the negative side of zero.

These experiments, Gentlemen, corroborate a result which to my mind is sufficiently well established without them. Nevertheless I would say, that the conclusions of Mr. Adie are such as a restricted examination of the subject will most probably lead to. I have no doubt as to the correctness of his results described in the September Number of the Magazine; but I have just as little doubt, that had Mr. Adie varied the strength of his current sufficiently, he would have spared himself the statement, that "in his experiments he had never met a fact which in the least encourages the view that electricity reduces temperatures."

<div align="center">

I remain, Gentlemen,

Your obedient Servant,

JOHN TYNDALL.

</div>

Queenwood College,
November 1852.

LXVII. *On the Dynamical Theory of Heat.* By WILLIAM THOMSON, *M.A., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow*[*].

[Continued from p. 176.]

PART IV.—*On a Method of discovering experimentally the Relation between the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid.*

61. THE important researches of Joule on the thermal circumstances connected with the expansion and compression of air, and the admirable reasoning upon them expressed in his paper[†] "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," especially the way in which he takes into account any mechanical effect that may be externally produced, or internally lost, in fluid friction, have introduced an entirely new method of treating questions regarding the physical properties of fluids. The object of the present paper is to show how, by the use of this new method, in connexion with the principles explained in my preceding paper, a complete theoretical view may be obtained of the phænomena experimented on by Joule; and to point out some of the objects to be attained by a continuation and extension of his experimental researches.

62. The Appendix to my Account of Carnot's Theory[‡] contains a theoretical investigation of the heat developed by the compression of any fluid fulfilling the laws[§] of Boyle and Mariotte and of Dalton and Gay-Lussac. It has since been shown that that investigation requires no modification when the dynamical Theory is adopted, and therefore the formula obtained as the result may be regarded as being established for a fluid of the kind assumed, independently of any hypothesis whatever. We may obtain a corresponding formula applicable to a fluid not fulfilling the gaseous laws of density, or to a solid pressed uniformly on all sides, in the following manner.

63. Let Mdv be the quantity of heat absorbed by a body kept at a constant temperature t, when its volume is increased from v to $v + dv$; let p be the uniform pressure which it experiences from without, when its volume is v and its temperature t; and let $p + \dfrac{dp}{dt} dt$ denote the value p would acquire if the temperature

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[*] From the Transactions of the Royal Society of Edinburgh, vol. xx. part 2. April 17, 1851.
[†] Philosophical Magazine, May 1845, vol. xxvi. p. 369.
[‡] Transactions, vol. xvi. part 5.
[§] To avoid circumlocution, these laws will, in what follows, be called simply the *gaseous laws*, or the *gaseous laws* of density.

were raised to $t + dt$, the volume remaining unchanged. Then, by equation (3) of § 21 of my former paper, derived from Clausius's extension of Carnot's theory, we have

$$M = \frac{1}{\mu} \cdot \frac{dp}{dt} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (a)*,$$

where μ denotes Carnot's *function*, the same for all substances at the same temperature.

Now let the substance expand from any volume V to V′, and, being kept constantly at the temperature t, let it absorb a quantity, H, of heat. Then

$$H = \int_V^{V'} M dv = \frac{1}{\mu} \frac{d}{dt} \int_V^{V'} p dv \quad \cdot \quad \cdot \quad (b).$$

But if W denote the mechanical work which the substance does in expanding, we have

$$W = \int_V^{V'} p dv \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (c),$$

and therefore

$$H = \frac{1}{\mu} \frac{dW}{dt} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (d).$$

This formula, established without any assumption admitting of doubt, expresses the relation between the heat developed by the compression of any substance whatever, and the mechanical work which is required to effect the compression, as far as it can be determined without hypothesis by purely theoretical considerations.

64. The preceding formula leads to that which I formerly gave for the case of fluids subject to the gaseous laws; since for such we have

$$pv = p_0 v_0 (1 + Et) \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1),$$

from which we deduce, by (c),

$$W = p_0 v_0 (1 + Et) \log \frac{V'}{V} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2),$$

and

$$\frac{dW}{dt} = Ep_0 v_0 . \log \frac{V'}{V} = \frac{E}{1 + Et} W \quad \cdot \quad \cdot \quad (3);$$

and therefore, by (d),

$$H = \frac{E}{\mu(1 + Et)} W \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4).$$

which agrees with equation (11) of § 49 of the former paper.

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* Throughout this paper, formulæ which involve no hypothesis whatever are marked with italic letters; formulæ which involve Boyle's and Dalton's laws are marked with Arabic numerals; and formulæ involving, besides, Mayer's hypothesis, are marked with Roman numerals.

65. Hence we conclude, that the heat evolved by any fluid fulfilling the gaseous laws is proportional to the work spent in compressing it at any given constant temperature; but that the quantity of work required to produce a unit of heat is not constant for all temperatures, unless Carnot's function for different temperatures vary inversely as $1 + Et$; and that it is not the simple mechanical equivalent of the heat, as it was unwarrantably* assumed by Mayer to be, unless this function have precisely the expression

$$\mu = J \cdot \frac{E}{1 + Et} \quad \ldots \ldots \ldots \ldots (I).$$

This formula was suggested to me by Mr. Joule, in a letter dated December 9, 1848, as probably a true expression for μ, being required to reconcile the expression derived from Carnot's theory (which I had communicated to him) for the heat evolved in terms of the work spent in the compression of a gas, with the hypothesis that the latter of these is exactly the mechanical equivalent of the former, which he had adopted in consequence of its being, at least approximately, verified by his own experiments. This, which will be called Mayer's hypothesis, from its having been first assumed by Mayer, is also assumed by Clausius without any reason from experiment; and an expression for μ the same as the preceding, is consequently adopted by him as the foundation of his mathematical deductions from elementary reasoning regarding the motive power of heat. The preceding formulæ show, that if it be true at a particular temperature for any one fluid fulfilling the gaseous laws, it must be true for every such fluid at the same temperature.

66. Of the various experimental researches which might be suggested as suitable for testing Mayer's hypothesis, it appears from the preceding formula, that any which would give data for the determination of the values of μ through a wide range of temperatures would, with a single accurate determination of J, afford a complete test. Thus an experimental determination of the density of saturated steam for temperatures from 0° to 230° Cent. would complete the data, of which a part have been so accurately determined by Regnault, for the calculation of the values of μ between those wide limits, and would contribute more, perhaps, than any set of experimental researches that could at present be proposed, to advance the mechanical theory of heat.

67. The values of μ, given in Table I. of my Account of Car-

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* In violation of Carnot's important principle, that thermal agency and mechanical effect, or mechanical agency and thermal effect, cannot be regarded in the simple relation of cause and effect, when any other effect, such as the alteration of the density of a body, is finally concerned.

not's Theory, which were calculated from Regnault's observations on steam, with the assumption of $\frac{1}{1693\cdot5}$, (the maximum density of water being unity) for the density of saturated steam at 100° Cent., and of the gaseous laws for calculating it by means of Regnault's observed pressures, at other temperatures, are far from verifying equation (I), as appears from the Table of the values of $\frac{\mu(1 + Et)}{E}$, given in the preceding paper, § 51 ; or as the following comparative Table shows :—

Col. 1.	Col. 2.	Col. 3.	Col. 4.
Temperature.	Values of μ according to assumed density of saturated steam.	Values of μ according to Joule's formula.	Values of μ according to modified assumption for density of saturated steam.
t.	$[\mu]$.	$J\frac{E}{1+Et}$.	$\frac{1717\cdot6}{1693\cdot5} \times [\mu]$.
0	4·967	5·087	5·038
10	4·832	4·908	4·901
20	4·703	4·740	4·769
30	4·578	4·584	4·643
40	4·456	4·438	4·519
50	4·337	4·300	4·399
60	4·221	4·171	4·281
70	4·114	4·050	4·172
80	4·013	3·935	4·070
90	3·921	3·827	3·977
100	3·833	3·724	3·887
110	3·753	3·627	3·806
120	3·679	3·535	3·731
130	3·611	3·447	3·662
140	3·546	3·364	3·596
150	3·487	3·284	3·536
160	3·432	3·209	3·481
170	3·382	3·136	3·430
180	3·335	3·067	3·382
190	3·289	3·001	3·336
200	3·247	2·937	3·293
210	3·208	2·876	3·254
220	3·171	2·818	3·216
230	3·135	2·762	3·179

Mr. Joule, when I pointed out these discrepancies to him in the year 1848, suggested that even between 0° and 100° the inaccuracy of the data regarding steam might be sufficient to account for them. I think it will be generally admitted that there can be no such inaccuracy in Regnault's part of the data, and there remains only the uncertainty regarding the density of saturated steam, to prevent the conclusion that μ cannot be expressed by $J\frac{E}{1+Et}$; so that Mayer's hypothesis would be confirmed if, and overturned unless, the density of saturated steam, instead of following the gaseous laws, were truly expressed by

the equations

$$\left.\begin{array}{c}
\sigma = \dfrac{\left(\dfrac{1}{E}+t\right)[\mu]}{J} \cdot [\sigma] \\[3mm]
[\sigma] = \dfrac{1}{1693 \cdot 5} \cdot \dfrac{1+E\times 100}{1+Et} \cdot \dfrac{p}{\Pi}
\end{array}\right\} \quad \ldots \quad \text{(II)},$$

where $[\mu]$ denotes the quantity tabulated for the temperatures
$0°, 1°, 2°, \ldots 230°$ in Table I. of my Account of Carnot's Theory;
and $[\sigma]$ denotes the density of saturated steam which was
assumed in the calculation of that table, the values of $\dfrac{p}{\Pi}$ in the
expression for it being obtained by dividing the numbers tabu-
lated at the end of Regnault's eighth Mémoire by 760. The
considerableness of the deviations from the gaseous laws which
equation (II) indicates, is seen at once by comparing the num-
bers in column 2 with those in column 3 of the preceding table,
and observing that the coefficient of $[\sigma]$ in (II) is, for each tem-
perature shown in that table, obtained by dividing the corre-
sponding number in column 2 by that in column 3. Column 4
shows what the values of μ would be if the density of saturated
steam at $100°$ were $\frac{1}{1717\cdot6}$ instead of $\frac{1}{1693\cdot5}$, and, for other tempe-
ratures, varied according to the gaseous laws.

68. This subject has been very carefully examined by Clausius,
who has indicated the great deviations from the gaseous laws of
density that Mayer's hypothesis requires in saturated steam, and
has given an empirical formula for the density of saturated steam
founded on that hypothesis, and on Regnault's observations on
the pressure and latent heat. In this direction theory can go
no further, for want of experimental data; although, from what
we know of gases and saturated vapours, it may be doubted
whether such excessive deviations, in the case of steam, from the
laws of a " perfect gas " are rendered probable by a hypothesis
resting on no experimental evidence whatever[*].

69. To Joule we are indebted for a most important series of
experimental researches on the relation between the thermal
effects, the external mechanical effects, and the internal mecha-
nical effects (*vis viva* destroyed by fluid friction) due to com-
pressions and expansions of air in various circumstances[†]. These

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[*] Joule's experimental verification of Mayer's law for temperatures of
from $50°$ to $60°$ Fahr. shows, if rigorously exact, that the density of satu-
rated steam at about $10°$ Centigrade must be $\frac{1693\cdot5}{1717\cdot6}$ of what was assumed for
it in the calculations of my former paper, but does not go towards indica-
ting any deviation from the gaseous laws of variation in the density of satu-
rated steam at different temperatures.

[†] *Philosophical Magazine*, May 1845.

researches afford actual tests, which, so far as they go, are veri-
fications of the truth of Mayer's hypothesis for temperatures
between 50° and 60° Fahr., founded on two distinct methods,
either of which is perfect in principle, and might be made the
foundation of experiments at any temperature whatever.

70. The first of these methods consists simply in determining,
by direct experiment, the heat evolved by the expenditure of a
given amount of work in compressing air, and comparing it with
the quantity of heat created by the same amount of work in
Joule's original experiments on the heat developed by magneto-
electricity, and by the friction of fluids in motion.

71. The second method is especially remarkable, as affording
in each experiment an independent test of the truth of Mayer's
hypothesis for air at the temperature used, without requiring any
knowledge of the absolute value of the mechanical equivalent of
heat.   In Joule's actual experiments, the test is simply this :—
the total external thermal effect is determined when air is allowed
to expand, through a small orifice, from one vessel into another
previously exhausted by an air-pump.   Here the first mechanical
effect produced by the expanding gas is *vis viva* generated in the
rushing of the air.   By the time equilibrium is established, all
this mechanical effect has been lost in fluid friction (there being
no appreciable mechanical effect produced externally in sound,
which is the only external mechanical effect, other than heat,
that can be produced by the motions of a fluid within a fixed
rigid vessel) ; and no truth in physical science can be more cer-
tain, than that by the time thermal as well as mechanical equi-
librium is established at the primitive temperature, the contents
of the two vessels must have parted with just as much more heat
than they would have parted with had the air in expanding
pushed out a piston against an external resisting force, as is
equivalent to the mechanical effect thus produced externally.
Hence if the two vessels and the tube connecting them be im-
mersed (as they are in Joule's first set of experiments with this
apparatus) in one vessel of water, and if, after time is allowed
for the pressure and temperature of the air to become the same
in the two vessels, the water be found to have neither gained nor
lost heat (it being understood, of course, that the air and all
other matter external to the water are at an absolutely constant
temperature during the experiment), then, for the temperature
of the experiment, Mayer's hypothesis is perfectly confirmed ;
but any final elevation or depression of temperature in the water
would show that the work due to the expansion is either greater
than or less than the absolute equivalent of the heat absorbed.

72. Mr. Joule's second experiment on the same apparatus, in
which he examined separately the external thermal effects round

each of the two vessels, and round a portion of the tube containing the small orifice (a stop-cock), has suggested to me a method which appears still simpler, and more suitable for obtaining an excessively delicate test of Mayer's hypothesis for any temperature. It consists merely in dispensing with the two vessels in Joule's apparatus, and substituting for them two long spirals of tube (instead of doing this for only one of the vessels, as Joule does in his third experiment with the same apparatus); and in forcing air continuously through the whole.   The first spiral portion of the tube, up to a short distance from the orifice, ought to be kept as nearly as possible at the temperature of the atmosphere surrounding the portion containing the orifice, and serves merely to fix the temperature of the entering air.   The following investigation shows what conclusions might be drawn by experimenting on the thermal phænomena of any fluid whatever treated in this manner.

73. Let p be the uniform pressure of the fluid in the first spiral, up to a short distance from the orifice, and let p' be the pressure a short distance from the orifice on the other side, which will be uniform through the second spiral. Let t be the constant external temperature, and let the air in both spirals be kept as closely as possible at the same temperature. If there be any elevation or depression of temperature of the fluid in passing through the orifice, it may only be after passing through a considerable length of the second spiral that it will again arrive sensibly at the temperature t; and the spiral must be made at least so long, that the fluid issuing from the open end of it, when accurately tested, may be found not to differ appreciably from the primitive temperature t.

74. Let H be the total quantity of heat emitted from the portion of the tube containing the orifice, and the second spiral, during the passage of a volume u through the first spiral, or of an equivalent volume u' through the parts of the second where the temperature is sensibly t. This will consist of two parts; one (positive) the heat produced by the fluid friction, and the other (negative) the heat emitted by that portion of the fluid which passes from one side to the other of the orifice, in virtue of its expansion. To find these two parts, let us first suppose the transference of the fluid to take place without loss of mechanical effect in fluid friction, as it would do if, instead of the partition with a small orifice, there were substituted a moveable piston, and if a volume u of fluid, on the side where the pressure is higher (p), were enclosed between that and another piston, and allowed to slide through the tube till the second piston should take the place of the first, and to expand till its volume should be u'. If we adopt the same notation with reference to

the volume, v, of the substance between the pistons, kept at a constant temperature, t, as has been used uniformly in this and the preceding paper; we shall have, for the quantity of heat absorbed during the motion of the piston,

$$\int_{u}^{u'} \mathrm{M}\,dv\,;$$

or, by the second fundamental equation of the theory, (3) of § 21 of the preceding paper,

$$\frac{1}{\mu}\int_{u}^{u'} \frac{d\varpi}{dt}\,dv,$$

where ϖ denotes the actual pressure (intermediate between p and p') of the substance when its volume is v. Again, the work done by the pistons will be given by the equation

$$\mathrm{W}=\int_{u}^{u} \varpi dv+pu-p'u' \quad \cdot \quad \cdot \quad \cdot \quad (e).$$

If now the transference of the substance from the one portion of the tube, where the pressure is p, to the other, where the pressure is p', take place through a small orifice, exactly that amount, W, of work will be lost as external mechanical effect, and will go to generate thermal *vis viva*: The quantity of heat thus produced will be

$$\frac{1}{\mathrm{J}}\left\{ \int_{u}^{u'} \varpi dv+pu-p'u' \right\}.$$

Hence the total quantity of heat emitted will be the excess of this above the amount previously found to be absorbed when the mechanical effect is all external; and therefore we have*

$$\mathrm{H}=\frac{1}{\mathrm{J}}\left\{ \int_{u}^{u'} \varpi dv+pu'-p'u' \right\}-\frac{1}{\mu}\int_{u}^{u'} \frac{d\varpi}{dt}dv \quad \cdot \quad (f).$$

Whatever changes of temperature there may actually be of the air in or near the orifice, this expression will give rigorously the total quantity of heat emitted by that portion of tube which contains the orifice and the whole of the second spiral during the passage of a volume u through the first spiral, or u' through any portion of the second spiral where the temperature is sensibly t.

75. To apply this result to the case of a gas fulfilling the gaseous laws, we may put

$$pu=p'u'.$$

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* A more comprehensive investigation, including a proof of this result, is given in a subsequent communication (Royal Soc. Edinb. Dec. 15, 1851), constituting part 5 of the present series of articles, which will be republished in an early Number of this Journal.

Hence (*e*) becomes

$$W = \int_u^{u'} \varpi\, dv = pu \log\frac{u'}{u} = p'u'\log\frac{p}{p'} \quad . \quad . \quad (5),$$

and, by (3), we have

$$\frac{dW}{dt} = \frac{E p u}{1 + Et} \log\frac{u'}{u} = \frac{EW}{1 + Et}.$$

Hence the expression (*f*) for the heat emitted becomes

$$H = \left\{\frac{1}{J} - \frac{E}{\mu(1 + Et)}\right\} W \quad . \quad . \quad . \quad . \quad (6).$$

76. Lastly, if Mayer's hypothesis be fulfilled for the gas used in the experiment, the coefficient of W vanishes by (I.), and therefore

$$H = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (III).$$

77. From equation (III) it follows, that if Mayer's hypothesis be true, there is neither emission nor absorption of heat, on the whole, required to reduce the temperature of the air after passing through the orifice to its primitive value, *t*. Hence, although no doubt those portions of the air in the intermediate neighbourhood of the orifice which are communicating, by their expansion, *vis viva* to those contiguous to them will be becoming colder, and those which are the means of occasioning the portions contiguous to them to lose *vis viva*, through fluid friction, will be becoming warmer at each instant; yet very near the orifice on each side, where the motion of the air is uniform, the temperature would be constantly equal to *t*. Hence the simplest conceivable test of the truth of Mayer's hypothesis would be, to try whether the temperature of the air is exactly the same on the two sides of the orifice. This might be done by very delicate thermometers adjusted in the tube at sufficient distances on each side of the orifice to be quite out of the *rush* which there is of air in the immediate neighbourhood of the orifice; but it might be done in a still more refined manner by means of a delicate galvanometer, and a small thermo-electric battery arranged so that one set of the solderings might be within the tube on the side of the entering current of air, and the other set within the tube on the side of the current from the orifice. The tube on each side of the orifice would need to be bent so as to bring two parts of it, at small distances from the orifice on each side, near enough one another to admit of the battery being so placed. The only difficulty I can perceive in the way of making the necessary arrangements is what might be experienced in fitting the two ends of the battery air-tight into the two parts of the tube. It first occurred to me that the little battery itself might be placed entirely within the tube, and the difference of pressure

kept up in the two parts by the middle of the battery being fitted nearly air-tight in the tube by means of wax, or otherwise; but this arrangement would not be satisfactory, as portions of the bars of the battery, if not the ends themselves directly, would be altered in temperature, even if Mayer's hypothesis were rigorously true, on account of the rushing of the air among them. No part of the battery ought to be exposed to the rushing of the air in the neighbourhood of the orifice, and therefore the middle of the battery would have to be external to the tube, the ends being cemented into the tube by some indurating cement sufficiently strong and compact to hold perfectly air-tight on the side where the pressure is different from the atmospheric pressure. By such means as these, I think a very satisfactory series of experiments might easily be performed to test Mayer's hypothesis for air through a very wide range of temperatures.

78. Should the differential method of experimenting just described indicate any difference of temperature whatever on the two sides of the orifice, Mayer's hypothesis would be shown to be not exactly fulfilled, and, according as the air leaving the orifice is found to be warmer or colder than the entering air, we should infer that the heat absorbed, when air expands at a constant temperature, is less than or greater than the equivalent of the mechanical effect produced by the expansion *.

79. Calorimetrical methods, like those used by Joule, might then be followed for actually determining the heat emitted or absorbed by the air in the neighbourhood of the orifice, or in the second spiral, in acquiring the temperature of the air in the entering stream; and by careful experimenting, it is probable that excessively accurate results might be thus obtained for a wide range of temperature.

80. The result of each experiment would be a value of μ, in terms of Joule's mechanical equivalent, to be calculated by the following expression, derived from equations (5) and (6).

$$\mu = \frac{\dfrac{JE}{1+Et}}{1 - J \cdot \dfrac{H}{p'u' \log \dfrac{p}{p'}}} \quad \cdots \quad (7).$$

* Experiments on the plan here suggested have been recently made by Mr. Joule and myself, and it has thus been ascertained that the air leaves the *rapids* in the neighbourhood of the orifice at a lower temperature than it approached them, even if this temperature be as high as 170° F.; and it follows that the heat absorbed is *greater* than the equivalent of the mechanical effect of the expansion, even for so high a temperature, and probably for much higher. See a paper published in the Supplement to this Volume of the Magazine, in which these experiments are described.—Nov. 11, 1852.

In the second member of this equation p' denotes the pressure of the air through the second spiral, which would be the atmospheric pressure, or excessively near it, if, as in Joule's third experiment mentioned above (described by the author in p. 378 of the volume* containing his paper), the air leaving the second spiral be measured by means of a pneumatic trough: p denotes the pressure in the first spiral, which ought to be constant, and must be carefully measured; u' denotes the volume of air which leaves the apparatus in any time; and H denotes the quantity of heat emitted in the same time. The experiment might be continued for any length of time, and each one of these four quantities might be determined with great accuracy, so that probably very accurate direct results of observations might be obtained. If so, no way of experimenting could be better adapted than this to the determination of Carnot's function, for different temperatures, in terms of Joule's mechanical equivalent of heat.

LXVIII. *On the possibility of solving Equations of any degree however elevated.* By G. B. JERRARD, *Esq.*

[Continued from vol. iii. p. 460.]

§ 5.

I DO not think it necessary, after what has been already said, to state explicitly the objection to Abel's inference; but I cannot dismiss the subject without referring the reader to an admirable disquisition on equations the roots of which have a given relation among themselves in the *Mémoires de Mathématiques of* M. Libri.

We might now return to the general equation of the mth degree. Before, however, resuming the inquiry with which we set out, I purpose to show how to complete the method, given in my Mathematical Researches, of transforming equations by means of symmetric functions. This method, which cannot be explained in few words, will form the subject of a separate paper.

Long Stratton, Norfolk,
　August 27, 1852.

Erratum in vol. iii. p. 457, line 37.

For will admit *read* will, when the roots are unequal, admit. *The case of μ equal roots is not considered by Abel.*

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* Phil. Mag. vol. xxvi.

LXIX. *On Indirect Demonstration.* By Professor DE MORGAN*.

UNDER the phrase *indirect* demonstration, mathematicians are accustomed to include two things which are quite distinct. From this use of language springs confusion between the different characters of different methods. Geometers have seldom been very *formal* logicians; and their patent of exemption was signed by Euclid.

Indirect demonstration, as commonly conceived, means demonstration of the impossibility of all contradiction. But the following distinctions are required. Let the proposition to be proved be Every A is B. To avoid using *direct* in two senses, as opposed to *converse*, and as opposed to *indirect*, I shall take the correlatives *positive* and *contrapositive*, *direct* and *indirect*.

1. The *direct positive* proposition is Every A is B. The direct positive proof takes *any* A, and shows that it *is* B.

2. The *direct contrapositive* proposition, identical with the last, is Every not-B is not-A. The direct contrapositive proof takes any not-B, and shows that it *is* not-A.

3. The *indirect positive* proof attacks the *positive* contradiction, Some As are not-Bs, and taking an A assumed to be not-B, shows the assumption to have an absurdity for its necessary consequence.

4. The *indirect contrapositive* proof attacks the *contrapositive* contradiction, Some not-Bs are As, and taking a not-B assumed to be A, shows the assumption to have an absurdity for its necessary consequence.

The third and fourth have a slightness of distinction which I maintain to exist also as to the first and second. Applying the notion† of form and matter to *forms*, the first and second differ in form, and also the third and fourth. But the first pair are opposed to the second pair. The latter pair proceed from denial of consequence to denial of hypothesis: the former pair proceed from establishment of hypothesis to establishment of consequence.

When the mathematician uses the second form, he usually employs the third or fourth, subordinately, to connect it with the first. Is this necessary?

When we say a square is entirely contained within a circle, do we need an indirect process to establish that outside the circle is outside the square? Surely any attempt to establish this by

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* Communicated by the Author.

† The algebraist ought to be well accustomed to this application. In arithmetic 1, 2, 3, &c. are of the form, yards, gallons, &c. are of the matter. In common algebra, 1, 2, 3, &c. become the matter, and $a+b$, ab, &c. are distinctions of form. In higher algebra $a+b$, ab, &c. become material, and ϕ in $\phi(a+b)$, &c. belongs to the form. The distinction of form and matter is often concealed under the distinction of general and specific matter.

indirect process contains postulates of reasoning as difficult as the required transformation, if not of its very nature. Euclid would not apply the indirect process to prove the conclusion about a *space-area*: but he does apply it when the area is what logicians call the *extent* of a term. When A is entirely within B, species within genus, he never admits that all the notions outside the genus are outside the species, without an indirect demonstration. From Every not-B is not-A he produces Every A is B, thus:—If it be possible, let this A be not-B, but every not-B is not-A, therefore this A is not-A, which is absurd: whence every A is B. He might as well argue into the conclusion of a common syllogism from the premises, as thus;—Every A is B, this is an A, therefore it is a B; for if not let it be not-B, then one not-B is A, but every A is B, therefore not-B is B, which is absurd, &c. Here it is manifest that our reasoning takes fully as much for granted as the direct transition from premises to conclusion: we take syllogism for granted in proving syllogism. Euclid does more: he takes syllogism for granted in proving the antesyllogistic conversion of propositions. This does well for beginners, to whom simple affirmative syllogism is more familiar than conversion by contraposition: but I am now speaking to mathematicians who examine the laws of thought.

It is an easily ascertained fact, that really indirect demonstration is uncommon in geometry, except as a (to a logician) unnecessary help to contrapositive directness of proof. Take for example, Book I. Prop. 6. A *non-isosceles* triangle is *unequally* angled (at the base). Now i. 4 is, in one of its contrapositive forms, as follows. Two sides severally equal to two sides, with unequal areas, have unequal angles contained. Euclid's construction instantly brings out of a non-isosceles triangle two triangles with two sides severally equal to two sides, and areas in the relation of whole and part. Hence follows that a non-isosceles triangle is unequally angled at the base: to the logician this is identical with Euclid's form, Equal angles at the base give equal sides: the geometer who is not a logician is helped over this last step by the addition of an indirect demonstration.

Seeing that this so-called indirect proof, then, is in its indirect part seldom anything except the demonstration of the passage from contrapositive to positive, for the benefit of those to whom this step of *pure logic* is of uneasy transition, we may ask how the necessity for the contrapositive form is to be explained? The refutation of contradiction is viewed by some geometers as a kind of lame and imperfect proof. It is, indeed, mostly superfluous; but it is rather a *crutch* proof than a *lame* proof, when applied only to help in the conversion of a proposition. With reference, however, to the unavoidable entrance of both the direct forms, it

seems that the contrapositive proposition is often more accessible than the positive one, because we know more about the negative terms than about the positive ones : and we have to proceed from the more known to the less known.   It is surely no great wonder, and no cause of complaint against the nature of things, that we should sometimes find ourselves in a position in which we can only proceed to comparison of equals by previous comparison of unequals.   On the contrary, it seems clear to me that it should rather be matter of surprise that we are not obliged to do something yet more specific in the way of departure from consideration of equality.

The relations of magnitude (ratios) are infinite in number. If there were a person well versed in the truths of geometry and arithmetic, but wholly ignorant of their systematic derivation from each other, and if this person were informed that he must proceed to study demonstration, he would imagine that his earliest instrument would be —ratio in all its varieties.   He would be surprised when he was told that, for a considerable time, he would not be required to subdivide ratio into more than three cases, ratio of equality, and the two forms of ratio of inequality without any specification of the *degree of inequality*.   But perhaps he would be more surprised if he were told that, after this renunciation of the different modes of inequality, geometers were still unsatisfied whenever they had to reason from inequality to equality.   And if he were a logician, though by my supposition one who had not applied his logic in mathematics, he would be most surprised to know that geometers never made the contrapositive conversion of the universal affirmative except by an indirect demonstration, and laid the blame on the essential character of geometry, instead of laying it on their own neglect of the study of the pure laws of thought, as they apply in geometry and everything else.

I have been led to offer these remarks at this particular time by Mr. Sylvester's paper contained in your last Number, as to which I agree almost entirely with all that is Mr. Sylvester's own, and differ only as to the view of the indirect proof which he holds in common with most other geometers.   I cannot answer his invitation or challenge, because he will perhaps insist upon my passing from the contrapositive to the positive form only by an indirect demonstration.   But I claim to see identity in Every A is B and every not-B is not-A, by a process of thought prior to syllogism : and, proving that the inequality of the nearer segments makes the inequality of the remoter ones follow, I conclude that the equality of the remoter ones makes the equality of the nearer ones follow, as a new logical form of the preceding conclusion, identical with it in meaning.   Of

course it will be seen that I hold the direct contrapositive **proof** to be of a different character from the direct positive proof. What I have endeavoured to show is, that the difference of character is not that which geometers in general attribute when they lay stress upon the indirect proof by which they turn one form of logic into another identical with it.   So soon as a geometer shall find out that he wants proof, as to a square inside a circle, that what is out of the circle is out of the square, then, and not before, will he be entitled to insist on the logician *proving* that what is out of the genus is out of the species.

I do not intend the preceding criticism to imply that I would make any great change in Euclid.   The best way to learn separation is practice upon a mixed material, not observation of the separation as already made.   A teacher may, and should, call the attention of his pupil to the distinction of the form of thought and the matter thought on : but the compound product is the material on which he has to work, and this is presented by Euclid in its most natural form.

November 1, 1852.

LXX. *On the Principles of Hydrodynamics.   By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

[Continued from vol. i. p. 241.]

THE exposition of the principles of hydrodynamics which I commenced in the Number of this Magazine for January 1851, and continued in that for March of the same year, I now propose to resume, having been prevented by failure of health and want of leisure from returning to the subject at an earlier period.   The propositions contained in the two former communications will be referred to as proved, and the notation there adopted will still be employed, without further indication of the meanings of the symbols.

The first eight propositions, which were of a general nature, applying equally to all perfect fluids, were followed by one which related especially to incompressible fluids, and was thus enunciated : " To determine the law of action of the parts of an incompressible fluid on each other."   The use of this proposition in the solution of a few problems of fluid motion was then exemplified.   I proceed next to the consideration of an analogous proposition relating to a compressible fluid ; it being essential, according to the views already advocated, to deduce the laws of the mutual action of the parts of the fluid on each other previous to any determination of the circumstances of particular instances

of motion. The following principle will be found to be of assistance in this inquiry :—The general hydrodynamical equations being assumed to be exact and sufficient, any analytical circumstances which admit of interpretation with respect to the motion *prior* to the consideration of arbitrary cases of disturbance, have reference to the law of action of the parts of the fluid on each other.

Proposition X. It is required to determine the law of the mutual action of the parts of a compressible fluid, the pressure of which varies in the same proportion as the density.

(1.) The following equation was obtained in the proof of Proposition VI. (Phil. Mag. for January 1851, p. 33), viz.

$$\lambda(d\psi) = udx + vdy + wdz.$$

Now by an abstract theorem of analysis, the right-hand side of this equality is integrable if λ be a function of ψ, or more generally, a function of ψ and t. The same quantity is integrable in an unlimited number of ways by particular values of u, v, and w, depending on particular arbitrary disturbances. But the supposition that λ is a function of ψ and t is of a general nature, and may be made prior to the consideration of any case of motion. Hence, according to the principle above enunciated, if this supposition conducts to a result compatible with fluid motion, that result is indicative of the mode of action of the parts of the fluid on each other. But by Proposition VII. it was shown, that if λ be a function of ψ and t, the motion is rectilinear. Consequently, if the mode of action of the parts of the fluid on each other be such as to satisfy the condition of making $udx + vdy + wdz$ integrable, the motion is rectilinear.

At this stage of the reasoning it will be necessary to refer to the results which were obtained in the January Number (1851), by a consideration of rectilinear motion perpendicular to a fixed plane, and rectilinear motion tending to or from a fixed centre. (Examples I. and II. p. 34–37.) In each of these cases of motion absurd results were arrived at by reasoning strictly in accordance with the received principles of hydrodynamics. As those principles are not untrue, it hence follows that they are insufficient for the solution of hydrodynamical problems. Also, as the contradictory results were deduced from true principles, it is certain that the reasoning involved some false step, which it is essential to discover. Where the error was committed will appear in the course of the following investigation.

If the motion be in directions perpendicular to a fixed plane, and be a function of the distance from the plane, it will be rectilinear motion, and will satisfy the condition of making $udx + vdy + wdz$ integrable. May we, therefore, suppose that the parts

of the fluid so act on each other, that a motion of this kind results? The absurdity to which, as already stated, this supposition conducts, proves that it is not allowable. Again, motion which tends to or from a fixed centre, and is a function of the distance from the centre, is rectilinear motion, and satisfies the criterion of integrability of $udx + vdy + wdz$. The absurdity, however, to which the supposition of such motion conducts proves that this is not the kind of motion resulting from the mutual action of the parts of the fluid. Neither can it be motion tending to or from *focal lines*; for if this were the general law, no absurdity would result in the particular case of motion tending to or from a centre. Thus the absurd results above cited are extremely important, as excluding from our consideration the kinds of motion just mentioned.

(2.) It remains to consider the case of an *axis* of rectilinear motion. The general integrability of $udx + vdy + wdz$ is in this case only satisfied by the motion along, or immediately contiguous to, the axis, the motion at all other points being curvilinear. For the purpose of tracing the consequences of this supposition, let

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz},$$

f being a function of x and y only, and ϕ a function of z and t only. Further, let the function f be such, that where $x = 0$ and $y = 0$, we have

$$f = 1, \quad \frac{df}{dx} = 0, \quad \frac{df}{dy} = 0.$$

It is clear that on these suppositions $udx + vdy + wdz$ is integrable, and that the axis of z is a line of motion. If no contradictory results, similar to those before indicated, be arrived at by tracing the consequences of the above suppositions, the motion due to the action of the parts of the fluid on each other must be of the kind here assumed, because it is certain, *à priori*, that that motion is unique and perfectly definite.

Now as a first consequence of our hypothesis, we have

$$(d.f\phi) = udx + vdy + wdz. \quad \cdots \quad (\alpha)$$

Combining with this equality the general equation of Proposition IV., viz.

$$\frac{d\rho}{dt} + \frac{d.\rho u}{dx} + \frac{d.\rho v}{dy} + \frac{d.\rho w}{dz} = 0, \quad \cdots \quad (\beta)$$

and that which the general equation of Proposition V. becomes when there are no impressed forces, viz.

$$\frac{a^2.(d\rho)}{\rho} + \left(\frac{du}{dt}\right)dx + \left(\frac{dv}{dt}\right)dy + \left(\frac{dw}{dt}\right)dz = 0, \quad \cdot \quad (\gamma)$$

and then eliminating by a known process ρ, u, v, w from the three equations (α), (β), (γ), the result will be,

$$
\left.
\begin{aligned}
\mathrm{F}'(t) = a^2 . \Bigg\{ & \phi\left(\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2}\right) + f\frac{d^2\phi}{dz^2} \Bigg\} - f\frac{d^2\phi}{dt^2} \\
& - 2\phi\left(\frac{d\phi}{dt} + f\frac{d\phi^2}{dz^2}\right)\left(\frac{df^2}{dx^2} + \frac{df^2}{dy^2}\right) \\
& - \phi^3\left(\frac{df^2}{dx^2} . \frac{d^2f}{dx^2} + 2\frac{df}{dx} . \frac{df}{dy} . \frac{d^2f}{dxdy} + \frac{df^2}{dy^2} . \frac{d^2f}{dy^2}\right) \\
& - 2f^2\frac{d\phi}{dz} . \frac{d^2\phi}{dzdt} - f^3 . \frac{d\phi^2}{dz^2} . \frac{d^2\phi}{dz^2}
\end{aligned}
\right\} . \quad (\delta)
$$

But since, from what has already been said, this equation applies only to points on the axis of z, or immediately contiguous to it, the terms involving $\frac{df}{dx}$ and $\frac{df}{dy}$ will be infinitely less than the other terms. And again, as the value $f=1$ results from the values $x=0$, $y=0$, which make $\frac{df}{dx}=0$, and $\frac{df}{dy}=0$, it follows that that value of f is either a maximum or a minimum. The supposition of a minimum leads to contradictory results, and is by that circumstance excluded from the investigation. Hence, supposing that

$$
\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} = -\frac{b^2}{a^2},
$$

and omitting in (δ) the terms involving $\frac{df}{dx}$ and $\frac{df}{dy}$, we have for the motion along the axis,

$$
\mathrm{F}'(t) + b^2\phi - a^2\frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} + 2\frac{d\phi}{dz} . \frac{d^2\phi}{dzdt} + \frac{d\phi^2}{dz^2} . \frac{d^2\phi}{dz^2} = 0. \quad (B)
$$

The arbitrary quantity $\mathrm{F}'(t)$ disappears if $\mathrm{F}(t)$ be supposed to be zero or a constant, and this supposition is required by the motion which is the subject of this investigation, which is independent of any arbitrary circumstance. Omitting, therefore, $\mathrm{F}'(t)$, the equation just obtained possesses the remarkable property of being satisfied by motion along the axis, such that the density and velocity existing at any instant at any point are propagated without alteration at a certain uniform rate. This property I proceed to demonstrate.

On the supposition that the motion is of the kind above described, the density (ρ) must satisfy the equation

$$
\frac{d\rho}{dt} + a_1\frac{d\rho}{dz} = 0,
$$

a_1 being the constant rate of propagation. For the integral of this equation is $\rho = \mathrm{F}(z - a_1 t)$. Now, since

$$\frac{d\rho}{dt} + \frac{d \cdot \rho u}{dx} + \frac{d \cdot \rho v}{dy} + \frac{d \cdot \rho w}{dz} = 0,$$

by substituting from the equation above, we have

$$a_1 \frac{d\rho}{dz} = \frac{d \cdot \rho u}{dx} + \frac{d \cdot \rho v}{dy} + \frac{d \cdot \rho w}{dz}.$$

Since $u = 0$ and $v = 0$ for the motion along the axis of z, this equation becomes

$$(a_1 - w)\frac{d\rho}{\rho dz} = \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}.$$

Also, because $u = \phi \dfrac{df}{dx}$, $v = \phi \dfrac{df}{dy}$, $w = f\dfrac{d\phi}{dz}$, $f = 1$, and

$$\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} = -\frac{b^2}{a^2},$$

we obtain by substituting,

$$\left(a_1 - \frac{d\phi}{dz}\right)\frac{d\rho}{\rho dz} = -\frac{b^2 \phi}{a^2} + \frac{d^2 \phi}{dz^2}. \quad \cdots \quad (\epsilon)$$

The known general equation which gives the value of the density, becomes for the motion along the axis of z,

$$a^2 . \mathrm{Nap.\ log\ } \rho + f\frac{d\phi}{dt} + \frac{f^2}{2} \cdot \frac{d\phi^2}{dz^2} = \mathrm{F}(t).$$

Differentiating with respect to z, and putting $f = 1$,

$$\frac{a^2 . d\rho}{\rho dz} + \frac{d^2 \phi}{dz dt} + \frac{d\phi}{dz}\frac{d^2 \phi}{dz^2} = 0.$$

Eliminating ρ from (ϵ) by this last equation, the result is

$$-b^2 \phi + a^2 . \frac{d^2 \phi}{dz^2} + \left(a_1 - \frac{d\phi}{dz}\right)\left(\frac{d^2 \phi}{dz dt} + \frac{d\phi}{dz} \cdot \frac{d^2 \phi}{dz^2}\right) = 0;$$

by comparison of which equation with the equation (B), we obtain

$$\frac{d^2 \phi}{dt^2} + a_1 \frac{d^2 \phi}{dz dt} + \frac{d\phi}{dz} \cdot \left(a_1 \frac{d^2 \phi}{dz^2} + \frac{d^2 \phi}{dz dt}\right) = 0. \quad . \quad (\theta)$$

Now this equation vanishes identically if

$$\frac{d\phi}{dt} + a_1 \frac{d\phi}{dz} = 0,$$

that is, if ϕ, $\dfrac{d\phi}{dz}$, and $\dfrac{d\phi}{dt}$ be functions of $z - a_1 t$, and by consequence ρ be a function of the same quantity, a result in accordance with the original hypothesis respecting ρ.

Let us, therefore, trace the consequence of introducing the

condition $\phi = F(z - a_1 t)$ into the equation (B). Writing for the sake of brevity ν for $z - a_1 t$, and F for $F(\nu)$, we shall have

$$\frac{d^2\phi}{dt^2} = a_1{}^2 \frac{d^2F}{d\nu^2}, \quad \frac{d\phi}{dz} = \frac{dF}{d\nu}, \quad \frac{d^2\phi}{dz\,dt} = -a_1 \frac{d^2F}{d\nu^2}.$$

Consequently, by substituting in (B),

$$\frac{d^2F}{d\nu^2}\left(a_1{}^2 - a^2 - 2a_1\frac{dF}{d\nu} + \frac{dF^2}{d\nu^2}\right) + b^2 F = 0.$$

This equation may be integrated by successive approximations proceeding according to the powers of F. To the first approximation,

$$\frac{d^2F}{d\nu^2} + \frac{b^2}{a_1{}^2 - a^2}\cdot F = 0.$$

Hence by integrating,

$$F = m \cos(q\nu + c),$$

where q is substituted for $\dfrac{b}{\sqrt{a_1{}^2 - a^2}}$, so that $a_1{}^2 = a^2 + \dfrac{b^2}{q^2}$. Consequently

$$\phi = m \cos q\left(z - at\sqrt{1 + \frac{b^2}{q^2 a^2}} + c\right).$$

By proceeding to the third approximation, I find the following results:—

$$\phi = m \cos q(z - a_1 t + c)$$
$$- \frac{m^2 q^3 a_1}{3b^2}\sin 2q(z - a_1 t + c)$$
$$- \frac{m^3 q^4}{4b^2}\left(\frac{q^2 a^2}{b^2} + \frac{7}{8}\right)\cos 3q(z - a_1 t + c)$$
$$a_1{}^2 = a^2 + \frac{b^2}{q^2} + m^2 q^2\left(\frac{2q^2 a^2}{3b^2} + \frac{5}{12}\right).$$

Having thus shown that the equation (B) is satisfied by the supposition of a uniform and identical rate of propagation at all points of the axis, and having found approximately the values of ϕ and a_1 to which this supposition leads, I proceed to consider the integral of that equation in a more general manner.

It does not appear that an exact integral of (B) can be obtained. An integral, however, applicable to the present inquiry is deducible as follows by successive approximations. For a first approximation, taking the terms of the first order with respect to ϕ, we have

$$\frac{d^2\phi}{dt^2} - a^2 \frac{d^2\phi}{dz^2} + b^2\phi = 0. \quad \dots \quad (\eta)$$

Putting, for convenience, e for $\dfrac{b^2}{4a^2}$, μ for $z+at$, and ν for $z-at$, the integral of the above equation in a series proceeding according to powers of e is

$$\begin{aligned}
\phi = {} & F(\mu) + G(\nu) \\
& + e \cdot \{\nu F_1(\mu) + \mu G_1(\nu)\} \\
& + \frac{e^2}{1.2} \cdot \{\nu^2 F_2(\mu) + \mu^2 G_2(\nu)\} \\
& + \&c.,
\end{aligned}$$

where

$$F_1(\mu) = \int F(\mu)d\mu, \quad F_2(\mu) = \int F_1(\mu)d\mu, \quad G_1(\nu) = \int G(\nu)d\nu, \ \&c.$$

As the functions F and G satisfy the equation (η) independently of each other, it is permitted to consider them separately. Let, therefore,

$$\phi = G(\nu) + e\mu G_1(\nu) + \frac{e^2\mu^2}{1.2} G_2(\nu) + \frac{e^3\mu^3}{1.2.3} \cdot G_3(\nu) + \&c.$$

This value of ϕ, containing arbitrary quantities, is not generally applicable to the present inquiry, which is antecedent to any case of arbitrary disturbance. It is, however, to be remarked that ϕ has particular forms, expressible in finite terms, if forms of the function G can be found, which will satisfy the equality

$$\frac{d.G_n(\nu)}{d\nu} = \pm k^2 G_{n+1}(\nu)$$

for every integral value of n. Now,

$$G_{n+1}(\nu) = \int G_n(\nu)d\nu.$$

Hence, by the above equality,

$$\frac{d.G_n(\nu)}{d\nu} = \pm k^2 \int G_n(\nu)d\nu,$$

or

$$\frac{d^2 G_n(\nu)}{d\nu^2} \mp k^2 G_n(\nu) = 0.$$

The upper sign gives a logarithmic form to the function G, which is incompatible with any general law of fluid motion, as also with the value of ϕ already obtained. Taking the lower sign and integrating, we have

$$G_n(\nu) = A \cos(k\nu + c),$$

which determines the form of the function G. In conformity

with this result, let $G(\nu) = m \cos(k\nu + c)$.   Then it will be found that

$$\phi = m \cos\left\{ k\left(\nu - \frac{e\mu}{k^2}\right) + c \right\}.$$

Or, putting q for $k - \dfrac{e}{k}$, and substituting the values of ν and μ,

$$\phi = m \cos q\left(z - at\sqrt{1 + \frac{4e}{q^2}} + c'\right).$$

By using this first approximate value of ϕ, and integrating (B) to the second and third approximations, exactly the same expressions for ϕ and a_1 result as those obtained by the former process.

Thus the hypothesis of an axis of rectilinear motion has been shown to be compatible with the hydrodynamical equations, no contradiction having been met with in the foregoing investigation. As this conclusion has been arrived at by the indications of the analysis prior to the consideration of any arbitrary case of disturbance, it may hence be concluded that the action of the parts of the fluid on each other is such, that there is always a rectilinear axis of motion along which the motion is *vibratory*, and that all the parts of a vibration are propagated with exactly the same velocity.

If instead of the function G we had reasoned with the function F, the same results would have been obtained, with the difference only that the propagation of the motion would have been in the opposite direction.   Hence as the equation (B) to the first approximation is satisfied by the sum of the values of ϕ, it follows that when the vibrations are small, two propagations may take place simultaneously along the axis in opposite directions.

(3.) Hitherto the reasoning has been carried on by means of exact equations, and some circumstances respecting the motion resulting from the mutual action of the parts of the fluid have been ascertained for velocities and condensations of any magnitude.   The laws of the curvilinear motion which takes place at finite distances from the axis of rectilinear motion, and which, as already stated, does not satisfy the condition of integrability of $udx + vdy + wdz$, can probably be arrived at only by successive approximations, commencing with terms of the first order with reference to the velocity and condensation.   The reasoning in future will be restricted to terms of the first order, so that the equations will be linear.

Now it may be proved as follows, that if terms of the first order only be retained, the quantity $udx + vdy + wdz$ is integrable for all distances from the axis of rectilinear motion.

Let the pressure at any point xyz at the time t be $a^2(1+\sigma)$, σ being a small quantity the powers of which above the first are

neglected. Then we have the known approximate equations

$$\frac{a^2 d\sigma}{dx} + \frac{du}{dt} = 0, \quad \frac{a^2 d\sigma}{dy} + \frac{dv}{dt} = 0, \quad \frac{a^2 d\sigma}{dz} + \frac{dw}{dt} = 0.$$

Hence by integration,

$$u = C - a^2 \int \frac{d\sigma}{dx} dt = C - a^2 \cdot \frac{d. \int \sigma dt}{dx}$$

$$v = C' - a^2 \int \frac{d\sigma}{dy} dt = C' - a^2 \cdot \frac{d. \int \sigma dt}{dy}$$

$$w = C'' - a^2 \int \frac{d\sigma}{dz} dt = C'' - a^2 \cdot \frac{d. \int \sigma dt}{dz},$$

where C, C′, C″ are functions of x, y, and z not containing the time. For all cases of motion in which no part of the velocity is independent of the time, for instance, cases of vibratory motion, we shall have C = 0, C′ = 0, C″ = 0. Hence substituting θ for $-a^2 \int \sigma dt$, it follows that

$$u = \frac{d\theta}{dx}, \quad v = \frac{d\theta}{dy}, \quad w = \frac{d\theta}{dz},$$

and consequently that $u dx + v dy + w dz$ is an exact differential. Since this inference has been drawn prior to the consideration of any specified case of motion, it must, according to our principles, be interpreted with reference to the motion resulting from the mutual action of the parts of the fluid. And as the inference depends on the assumption that no part of the motion is independent of the time, the physical circumstance indicated by the integrability of $u dx + v dy + w dz$ is, that the motion is *vibratory*. In accordance with this conclusion, the foregoing exact investigation of the motion along a rectilinear axis, so far as it is independent of any arbitrary forms given to the function F(t), is found to be vibratory motion.

Again, the new general hydrodynamical equation, viz.

$$\frac{d\psi}{dt} + \lambda \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) + \chi(t) = 0,$$

may be put under the form

$$\lambda \frac{d\psi}{dt} + u^2 + v^2 + w^2 + \lambda \chi(t) = 0;$$

which, if the squares of the velocities be neglected, becomes

$$\frac{d\psi}{dt} + \chi(t) = 0.$$

This equation gives by integration,

$$\psi + \chi_1(t) = D,$$

D being an arbitrary function of x, y and z. It cannot, therefore, be argued, as in Prop. VII., that ψ is a function of s and t, and by consequence that the motion is rectilinear. Hence the integrability of $udx + vdy + wdz$ for small values of u, v, w is consistent with curvilinear motion, and may be satisfied by the motion at any distance from the axis of rectilinear motion.

To carry on the investigation of the law of action of the parts of the fluid on each other to the first order of approximation, I shall continue to use the same expressions for the velocities as in the general case; but in consequence of what has just been proved, these expressions will not now be restricted to points contiguous to the axis of rectilinear motion. This extension of their application will be justified if it leads to no contradictory results. Thus we shall have

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz}, \quad . \quad . \quad . \quad . \quad (\iota)$$

at any point whose coordinates are x, y, z, and at any time t, f being a function of x and y only, and ϕ a function of z and t only.

The equation which gives the condensation σ to the first order of approximation is

$$a^2\sigma + f \frac{d\phi}{dt} = \mathrm{F}(t),$$

in which $\mathrm{F}(t)$ must be made to vanish in order that the reasoning may be conducted independently of any arbitrary circumstances. Consequently, after determining ϕ and f, the value of σ is given by the equation

$$a^2\sigma + f \frac{d\phi}{dt} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\kappa)$$

The equation (δ) to the first approximation becomes

$$a^2\phi \left(\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} \right) + a^2 f \frac{d^2\phi}{dz^2} - f \frac{d^2\phi}{dt^2} = 0. \quad . \quad . \quad (\lambda)$$

Now as ϕ is independent of x and y, it has the same value at all points of any plane perpendicular to the axis of z, and therefore the same value as at the point of intersection of this plane with the axis. But we have seen that for points on the axis the following equation is true to the first approximation, viz.

$$b^2\phi - a^2 \frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} = 0. \quad . \quad . \quad . \quad . \quad (\mu)$$

Hence substituting in (λ) from (μ), and striking out the common factor ϕ, we obtain

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + \frac{b^2 f}{a^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad (\nu)$$

We have thus arrived at an equation for determining f which is consistent with the original supposition that this quantity is a function of x and y only.

The next step is to ascertain the particular form of f which applies to the motion resulting from the mutual action of the parts of the fluid. As the equation (ν) is of the same form as the equation (μ), the same process that conducted to a particular solution of the latter must conduct to a particular solution of the former. In fact, by this process we obtain

$$f = \alpha \cos (gx + hy),$$

which evidently satisfies (ν), g and h being subject to the condition

$$g^2 + h^2 = \frac{b^2}{a^2} = 4e.$$

Let $g = 2\sqrt{e}\cos\theta$. Then $h = 2\sqrt{e}\sin\theta$, and the above integral may be put under the form

$$f = \alpha \cos \{2\sqrt{e}(x\cos\theta + y\sin\theta)\}. \quad . \quad . \quad (\pi)$$

Now as it was argued that an exact and unique integral of (μ), the form of which was indicated by the analysis, referred to the motion resulting from the mutual action of the parts of the fluid, by parity of reasoning, the integral (π) of the equation (ν) should receive the same interpretation. But it is to be observed, that since

$$u = \phi \frac{df}{dx} \text{ and } v = \phi \frac{df}{dy},$$

the value of f given by the equation (π) indicates that the part of the motion parallel to the plane of xy is parallel to an *arbitrary* direction in that plane depending on the value of θ. There is, however, an integral of (ν) which gets rid of this arbitrariness by embracing *all* directions depending on the arbitrary values of θ. For since that equation is linear with constant coefficients, it is clearly satisfied by supposing that

$$f = \Sigma \cdot \alpha\delta\theta \cos \{2\sqrt{e}(x\cos\theta + y\sin\theta)\},$$

$\delta\theta$ being an indefinitely small constant angle, and the summation being taken from $\theta = 0$ to $\theta = 2\pi$ in order to embrace every possible direction of the motion. By performing the summation, substituting r^2 for $x^2 + y^2$, and determining α so as to satisfy the condition that $f = 1$ where $r = 0$, the result is

$$f = 1 - er^2 + \frac{e^2 r^4}{1^2 \cdot 2^2} - \frac{e^3 r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c. \quad . \quad . \quad (\rho)$$

This value of f, containing no arbitrary quantity whatever, indi-

cates a general law of the spontaneous mutual action of the parts of the fluid. I have shown in the Philosophical Magazine for May 1849 (p. 363), that the same result is obtained by supposing the arbitrary functions in the general integral to be arbitrary constants. It may be worth while to indicate still another process by which the equation (ρ) may be deduced. Let

$$x + y \sqrt{-1} = \mu, \quad x - y \sqrt{-1} = \nu;$$

and in order to get rid of the impossible quantities, make F and G the same functions in the general integral of (ν). Then by supposing $F(\mu) = A\epsilon^{k\mu}$, and $G(\nu) = A\epsilon^{k\nu}$, ϵ being the base of the Napierian system of logarithms, the following exact value of f is found:

$$f = 2A\epsilon^{\left(k - \frac{e}{k}\right)x} \cos\left(k + \frac{e}{k}\right)y.$$

Let $x = r\cos\theta$, $y = r\sin\theta$; $k - \dfrac{e}{k} = m$, $k + \dfrac{e}{k} = n$; and expand the right-hand side of the above equation, viz.

$$2A\epsilon^{mr\cos\theta} \cos nr \sin\theta,$$

in terms arranged according to the dimensions of m and n. Then if, for the reason already alleged, the summation $\Sigma . f\delta\theta$ be taken from $\theta = 0$ to $\theta = 2\pi$, and the constant A be determined so that the sum shall be unity when $r = 0$, the result is

$$1 + (m^2 - n^2)\frac{r^2}{2^2} + (m^2 - n^2)^2 \cdot \frac{r^4}{2^2 \cdot 4^2} + \&c.,$$

which, since $m^2 - n^2 = -4e$, is independent of the arbitrary quantity k, and is plainly identical with the right-hand side of (ρ).

The values that have now been obtained for ϕ and f define precisely the motion along, and perpendicular to, the axis of rectilinear motion.

It may here be remarked, that a discussion of the equation (ρ) shows that f has an unlimited number of maximum values which become less as the distance from the axis is greater, and finally vanish at an infinite distance. Hence at an infinite distance f, $\dfrac{df}{dx}$, and $\dfrac{df}{dy}$ vanish, and by consequence the velocity vanishes. Hence also the condensation vanishes. Thus the supposition already made, that the arbitrary quantity $F(t)$ is equal to zero, is shown to be legitimate by the result of the preceding investigation, no part of which depends on that supposition. The condensation (σ) is therefore correctly given by the equation (κ), from which it is readily seen that we have also

$$\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + 4e\sigma = 0. \quad \ldots \ldots (\sigma)$$

As the equation (π) is a unique and exact integral of (ν)

obtained prior to the consideration of any case of motion, it ought, according to our principles, to have a *general* signification. It may be supposed to apply to motion which is *arbitrary* only so far as the angle θ is determined by some arbitrary circumstance. But its application is limited by the condition, that the motions obtained by giving particular values to θ are those only into which the original motion defined by the equation (ρ) may be resolved. As this last motion is dependent on no arbitrary circumstance whatever, it takes place equally under every *initial* disturbance, and any modification or resolution of it implies the operation of a *subsequent* disturbance. Let, if possible,

$$f = \alpha \cos \left\{ 2 \sqrt{e} (x \cos \theta + y \sin \theta) \right\}$$
$$+ \alpha' \cos \left\{ 2 \sqrt{e} \left(x \cos \overline{\frac{\pi}{2} + \theta} + y \sin \overline{\frac{\pi}{2} + \theta} \right) \right\}.$$

This is to suppose that the motion parallel to the plane of xy is at each point compounded of two motions in directions at right angles to each other. Expanding the above expression to second powers of x and y, we have

$$f = \alpha + \alpha' - 2\alpha e(x^2 \cos^2\theta + 2xy \sin \theta \cos \theta + y^2 \sin^2\theta)$$
$$- 2\alpha' e(x^2 \sin^2\theta - 2xy \sin \theta \cos \theta + y^2 \cos^2\theta).$$

By what has been said, this equality must be identical with

$$f = 1 - er^2 = 1 - e(x^2 + y^2).$$

Hence

$$\alpha + \alpha' = 1, \text{ and } \alpha = \alpha' = \frac{1}{2}.$$

Hence, as appears from equation (σ), if σ_1 and σ_2 be the condensations corresponding to the two motions into which the original motion is resolved, and S be the original condensation on the axis of z,

$$\sigma_1 = \frac{S}{2} \cos \left\{ 2 \sqrt{e} (x \cos \theta + y \sin \theta) \right\},$$
$$\sigma_2 = \frac{S}{2} \cos \left\{ 2 \sqrt{e} (y \cos \theta - x \sin \theta) \right\}.$$

If the expansions had been carried to higher powers of x and y, the two values of f would no longer have been identical. Hence we may infer that the solution (π) is applicable only to points very near the axis of motion; and that the motion which, for very small values of r, is defined by the equation $f = 1 - er^2$, may be resolved into two sets of motions, alike in all respects, but parallel to two planes at right angles to each other.

If these results be hypothetically applied to the undulatory theory of light, the original motion contiguous to the axis, and symmetrically disposed about it, corresponds to ordinary light, and the resolution of this motion corresponds to *polarization*.

[To be continued.]

LXXI. *On the relation of Magnetism and Diamagnetism to the Colour of Bodies.* By Richard Adie, *Esq., Liverpool**.

WHILE occupied with some experiments in the latter part of the year 1850, to test the magnetic properties of a variety of bodies, I was struck with the preponderance of transparent or white bodies among the class of diamagnetics; I consequently followed this branch of the inquiry, and in the following year gave, in Jameson's Edinburgh Journal, a table to prove that the diamagnetic metals produced a much larger proportion of colourless compounds than the magnetic ones. In the present instance I propose to return to the subject, to show that when the inquiry is confined to the oxides and chlorides, a similar relation subsists, although among the elementary bodies themselves there appears to be no connexion of the sort; indeed they tend to range themselves in an order opposite to that of the table I have alluded to, oxygen being magnetic and colourless, while chlorine, iodine, and bromine are diamagnetic and highly coloured.

It is in their compounds that the tendency of transparent substances to diamagnetism is seen, and in none is this more satisfactorily shown than in the most important of all the diamagnetics, water, for there 8 grs. of oxygen, a decidedly magnetic body, enter into combination with 1 gr. of hydrogen, a body of feeble diamagnetic properties, and produce 9 grs. of water, which the magnet repels, so that the magnetism of 8 parts of oxygen are more than counterbalanced by the diamagnetism of 1 part of hydrogen after their chemical union. Further, oxygen, where it forms colourless oxides with metals of very feeble magnetic properties, produces compounds where the magnetic power of the oxygen is masked, and there results a diamagnetic body like water; of these, Iceland spar, quartz and potash furnish instances.

In these cases, oxygen, although magnetic in itself, acts as a destroyer of magnetism in the new compounds; and if we turn to the oxides of the strongly magnetic metals, this property of oxygen is seen in a manner even more marked. For example, in the red oxide of iron a great amount of magnetic force is masked by the union of the oxygen with the iron.

The metals of marked magnetic or diamagnetic properties form but a small proportion in the general list of metallic bodies; three only can be said to be decidedly magnetic, namely, iron, nickel and cobalt; and four diamagnetic, bismuth, antimony, zinc and tellurium. Diamagnetism never assumes the power of magnetism; but in the four metals named, the property is sufficiently marked to manifest itself in the presence of minute im-

purities, which in the greater part of the other metals often determines whether the specimen tested is repelled or attracted.

On comparing the oxides and chlorides of the magnetic metals with those of the diamagnetic metals given, I find that there are three oxides and three chlorides of the magnetic metals all coloured; of the diamagnetic metals, there are three white oxides and one coloured, and four white chlorides* and one coloured, showing a great preponderance of white compounds to the diamagnetic substances.

Chlorine being a diamagnetic body, might be expected to give a greater proportion of colourless bodies among the chlorides than oxygen among the oxides of the same metals. To ascertain if this was so, I selected thirty-six of the metals which have been most examined, and tabulated their chlorides and oxides :—

Of the oxides, twenty-four were coloured and twelve white;

Of the chlorides, nineteen were coloured and seventeen white.

The chlorides, in conformity with this view, are found to furnish more white compounds than the oxides.

LXXII. *Non-polarization of the Aurora Borealis.* By WILLIAM JOHN MACQUORN RANKINE. *C.E., F.R.S.E., F.R.S.S.A., &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALTHOUGH the results which I have obtained are purely negative, it may be useful to record the fact, that having on several occasions during the last eight months examined the light of the aurora borealis with a Nichol's prism, I have never detected any trace of polarization. To show that this did not arise from the faintness of the light, I may mention, that on the last occasion when I observed it, the polarization of the same light produced by reflexion from the surface of a river was distinctly visible, although the direct light was evidently free from all sensible polarization.

This fact is adverse to the supposition that the light of the aurora borealis is reflected from crystals of ice.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, Nov. 22, 1852.   W. J. M. RANKINE.

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* One of these is a bichloride.

LXXIII. *On Copper Smelting.* By James Napier, *F.C.S.**

[Continued from p. 355.]

· *Calcination of the Ores.*

THE arranging and classifying the ores when brought into the smelting-works is a most important operation, affecting the whole after-workings, both as regards the cost and quality of the copper. The mines and localities from which the ore is brought are so numerous, that it would be impracticable to have a separate yard or compartment in the smelting-works for every one; hence a more general classification is adopted, such as highly sulphurous ores, including those containing mundic, fluccan or clayey ores coloured red or yellow, gray sulphur ores, &c. Sometimes certain mines or localities are known to produce ores of a certain quality; for instance those from Hayle have generally the character of being *stiff* (difficult to fuse); those from Foey Consols, of making inferior copper, &c.; these are all classified: then there are compartments for Cuba and Cobre, for the Irish ore, for Chilian ore, and for Australian ore; besides these qualities, the per-centage of copper in the ore has also to be considered: two or three lots of ores having respectively 7, 8, or 9 per cent. of copper, if their other qualities agree, may be put together, but not ores having 4, 8, and 12 per cent. The object of these arrangements and classifications of ores in the yard is to enable the operative smelter to select from and make up a constant working mixture, having the following characters :—

1st. That the copper in the mixture be not under 9 nor above 13 per cent.; if under the former, it is unprofitably poor; if over the latter, the slags have a tendency to contain copper, creating a loss.

2nd. That after being calcined an ordinary time, it will fuse easily without the necessity of adding flux, giving a clean and easily fused slag.

3rd. That the mat or coarse metal obtained from fusion contain as near as possible 30 per cent. of copper. And

4th. That the mixture do not contain ores having impurities calculated to make the copper of a lower quality than is desired.

There is no definite or fixed rule to guide the smelter in these classifications except a practised eye in distinguishing the character of ores, and the report of the assayer.

It must be borne in mind that we are speaking of ores containing sulphur; for although carbonates and oxides are brought

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into the condition of sulphurets in smelting, they are seldom mixed with the sulphuret ores, but with the calcined coarse metal from these ores to be described.

M. F. Le Play, in his *Procédés Métallurgiques,* has divided the ores into seven classes, and pointed out their distribution in the operations in the works as follows :—

1st. Ores containing from 3 to 15 per cent. of copper, mostly pyrites, with sulphuret of iron, and much earthy and other impurities.

2nd. Ores of the same character as the first, but richer, having from 15 to 25 per cent. of copper.

3rd. Ores requiring no calcination, having little sulphur and much oxide of copper, the per-centage of copper being between 12 and 20. .

4th. Ores composed of oxides and carbonates of copper, subsulphurets with small portions of pyrites, the matrix being oxide of iron and quartz.

5th. Same as the first class, but known to have no deleterious matters in them, such as tin, antimony, &c.

6th. Ores, principally subsulphurets, having a per-centage of from 60 to 80 of copper.

7th. The waste matters collected about the works termed, as we have already noticed, *cobbing.*

As this author describes ten operations as necessary in the manufacture of copper, his observations and descriptions must have reference to a period previous to the introduction of so much rich foreign ore, and some of these descriptions are therefore not applicable to the present time, the general routine of operations being now only six. The want of a definite rule to guide the smelter in mixing his ores is a desideratum we think easily supplied by investigation and attention. In the last section we mentioned that silica, alumina, &c. were by themselves infusible, but at high temperatures readily combine with other matters and form compounds that are easily fused; and in these combinations there are kinds and quantities of these substances much more easily fused than others. The first object of the smelter being to separate the metallic portion of the ore from the earths with the least expense and trouble, the having a mixture that will give him an easily flowing scoria, is a primary consideration. This, no doubt, could be easily obtained in the same way as shown in assaying, by adding flux; but every addition of flux is a loss in the smelting of copper, and should always be avoided if possible. In all combinations, to effect perfect fusion certain proportions are necessary to form a fusible compound, in the same way as in ordinary combinations. For example, to dis-

solve 32 grains of metallic copper in vitriol, it would require 49 grains of the strongest acid, and nothing less; hence 32 and 49 are their relative combining proportions. The same law holds good for every substance combining whether by fusion or solution. The equivalent or combining proportion of silica is 46. In order, then, to form a compound with silica that will be easily fused, we must employ equivalents of other substances. The following is a table of the combining proportions of those matters found in copper ores, or added as flux to fuse silica : —

Silica	46
Slaked lime	37
Carbonate of lime, chalk, shells, &c. .	50
Fluor-spar	39
Carbonate of barytes	98
Carbonate of magnesia	34
Carbonate of soda	53
Carbonate of potash	69
Protoxide of iron	36
Peroxide of iron	40
Oxide of copper	40
Oxide of lead	112
Oxide of tin	67

With these proportions, and an approximate analysis of the mixture of ores, a pretty accurate idea may be formed, before the matters are put into the furnace, of the kind of scoriæ that will be formed.

For the general character of the ores we refer to the tables in the first article, where it will be observed that silica is the principal earth present. Le Play gives the average composition of all the ores smelted in a work for a length of time as follows :—

Silica	38·5
Alumina	2·4
Lime	·3
Magnesia	·4
Copper	13·5
Iron	19·7
Other metals	·9
Sulphur	23·1
Oxygen, carbonic acid, and water	1·2
	100·0

The composition of the slag or scoriæ from fusing these ores is given by the same author as follows :—

Silica combined . . . . . 30·0
Silica mixed . . . . . . 30·5
Oxide of iron . . . . . 28·5
Alumina . . . . 2·9
Magnesia . . . . . . ·6
Different oxides . . . . 1·4
Lime . . . . 2·0
Fluoride of calcium . . . 2·1
Copper . . . . ·5
Iron . . . . ·9
Sulphur . . . . ·6
_____
100·0

By comparing this with the above analysis of ore, we find that lime and fluor-spar have been added to assist fusion, which should, when it is possible, be avoided; it will also be observed that the oxide of iron is the principal flux for the silica. The following table exhibits the general character and composition of different qualities of slag:—

	Silica.	Oxide of iron.	Lime.	Oxides of other metals.
Slag very difficult to fuse, having much silica *mixed*, requiring one-fourth longer time than usual to fuse each charge ...	70·7	28·4	1·2	
Slag a little stiff in working, *homogeneous*..................	63·1	35·9	·5	
Good thin slag, homogeneous	48·2	37·0	4·0	6
Good slag, easily fused	57·6	41·5	Trace.	
Slag good, and easily fused	55·2	38·5	·5	
Black glassy slag, easily fused	65	20·8	9·9	3 soda.

The first in the above table had pieces of quartz mixed with it, which is of very frequent occurrence in the slags, and may to a certain extent be calculated upon without prejudice to the fusion of the ores. Indeed, taking the average of analyses we have made of slags which the smelters term good working slags, and grinding the whole, mixed and combined quartz together, the weight of the protoxide of iron averages about one-half that of the silica. If we take the homogeneous slags, where the silica and iron are chemically combined, it will be observed how close the ingredients are to the table of proportions.

To follow the operation of calcination, let it be supposed that the mixture of ore has the following composition:—

Copper 13
Iron 29
Sulphur . . . 24
Silica 34
_____
100

Here there is more iron than is requisite to fuse the silica; but were this ore put into a fusing-furnace it could not be melted; the iron, copper, and sulphur would fuse together, leaving the unfused silica or quartz mixed up with it, forming an agglomeration, as silica will not combine with sulphurets of metals.   In the combination of matters in fusion we have often to be guided by the circumstances regulating affinity, as for example,—

> Iron requires a heat of about 3000° F. to melt it,
> Copper   ...   1900   ...

Sulphur is separated from these metals at a temperature of about 700° or 800°; but at a temperature of about 1200° the sulphur and these metals melt together, and their affinity for each other at this heat is increased.   Oxide of iron and quartz combine and fuse at about 1800°.   Bearing these conditions and relations in mind, it will be obvious that calcination is a primary and essential operation, the object of which is twofold.   The ore contains a great quantity of silica that must be got rid of by fusion; the best means for doing so is by oxide of iron.   The ore contains sufficient iron to effect this, which must also be got rid of; but the sulphur both retains and prevents it from combining with the silica; hence the operation of calcination is to drive away the sulphur and oxidate the iron, which is effected by exposing the ore to a temperature of from 700° to 1200°, so that the sulphur is separated from the metals and sublimed.   It is evident, therefore, that in mixing the ores for smelting, it is as necessary to mix them in relation to the iron they contain as to the earths.

Other circumstances have to be considered in mixing ores, namely, the presence of metals that would be injurious to the copper if not removed.   These are generally tin, antimony and arsenic; the latter does not deteriorate copper much, but it materially affects the operations of calcination and fusion.   Arsenic has a peculiar property over other metals in not having any fluid range; it remains solid until heated to about 356° F., when it passes off in a gaseous state, absorbing and carrying along with it a great quantity of heat which retards the operation of calcination; so that ores containing much arsenic should be sparingly mixed with other ores; or what is preferable, should be treated separately in the calcining operation, and mixed with other ores afterwards.   A large admixture of highly arsenious ores is a great drawback to the fusion, if the greater portion of the arsenic be not previously driven off.   We have known an extra expense of at least £20 per week for time, fuel and flux, to have been incurred during the smelting of a few hundred tons of ores containing much arsenic, under the impression that they were stiff and required flux, when an analysis would have shown that fluxes were

not required. But more money is spent in a year uselessly in the copper-works than would pay the salary of an efficient chemist, who could define the character of every ore before mixing, besides making other necessary inquiries which would be useful.

The mixture of ores being selected according to the rule adopted by the manager, it is carried to the large hoppers on the top of the calcining furnace and then let down into the hearth, where, after drying a little, it is spread equally over the bottom, covering it to a depth of from six to eight inches. The quantity of ore put in varies, according to the size of the furnace, from three tons to six tons. The fire of the furnace is kept low at first; after two or three hours the ore on the surface becomes visibly red, the heat is gradually increased to a yellow red; but this heat only penetrates to the depth of about an inch, consequently the ore has to be stirred and turned over by means of long iron paddles every hour, so as to expose a new surface to the action of the air and fire. This calcination lasts generally nine hours; but when ores are known to be stiff, containing much silica and sulphuret of iron, twelve hours are allowed. The following changes and chemical actions take place: the sulphur is partly burned off by combining with oxygen and forming sulphurous and sulphuric acids, and partly volatilized as sulphur uncombined; arsenic is volatilized either as metal or oxide; the copper and iron lose sulphur and combine with oxygen, which changes are subject to variations according to circumstances occurring in the operation.

In order to judge of the time necessary to calcine an ore, the nature and richness of the mat or coarse metal it will produce when fused has to be considered; and this does not depend upon the richness of the ore in copper when it goes into the calciner, but upon the quantity of sulphur and iron it contains. For instance, if we take the following ore and fuse it, adding flux to combine with the silica,—

Copper	.	.		22
Iron	.	.	.	31
Sulphur	.	.	.	32
Silica	.	.	.	15
				100

we should obtain a coarse metal or mat having only 26 per cent. of copper; and if we take another ore, having the following composition,—

Copper	.	.	.	11·2
Iron	.	.	.	18·4
Sulphur	.	.	.	12·4
Silica	.	.	.	58·0
				100·0

and fuse this in the same way, it would yield a coarse metal containing 30 per cent. of copper; so that here the poorest ore gives the richest mat. In order, then, to show the principle of mixing, and to obtain a fusible slag without flux, we will take two parts of the above rich ore and one of the poor ore, giving a mixture having the composition—

$$\begin{array}{ll}
\text{Copper} & 18\cdot4 \\
\text{Iron} & 26\cdot8 \\
\text{Sulphur} & 25\cdot5 \\
\text{Silica} & 29\cdot3
\end{array}$$

If this be calcined to volatilize half the sulphur, the remaining half, viz. 12·7, when the ore is fused, will combine thus :—

$$\begin{array}{lll}
4\cdot6 \text{ will combine with} & . & . & 18\cdot4 \text{ copper,} \\
8\cdot1 \text{ will take} & . & . & 14\cdot0 \text{ iron,}
\end{array}$$

producing a mat or coarse metal with 40 per cent. of copper. The oxide of iron being equal to 16·5 will combine with the 29·3 silica, forming the slag. We will enter more fully into these combinations further on.

With respect to the time any mixture of ore has to be calcined, the rate at which calcination proceeds has also to be considered, and forms a most important inquiry. Thus suppose the above mixture of ores lost half of its sulphur in nine hours, nine hours more would not suffice to drive off the remaining half.

We took a charge of Cuba ore and calcined twelve hours; samples taken out and tried every hour gave the following results :—

	Ore put in.	1 hour.	2 hours.	3 hours.	4 hours.	5 hours.	6 hours.	7 hours.	8 hours.	9 hours.	10 hours.	11 hours.	12 hours.
Copper...	12·3	13·0	12·2	12·2	13·0	12·2	13·8	12·6	12·6	12·5	13·2	13·8	12·2
Iron	32·7	30·0	24·4	32·8	28·7	31·3	33·6	30·6	30·0	27·6	24·3	40·3	27·0
Sulphur..	31·1	28·3	23·6	18·6	29·2	24·4	12·2	18·1	20·0	15·9	18·8	17·5	16·2
Silica ...	24·0	28·0	32·0	28·0	26·0	28·0	34·8	32·0	30·0	30·8	33·0	21·0	40·0
	100·0	99·3	92·2	91·6	96·9	95·9	94·4	93·3	92·6	86·8	89·3	93·6	95·4

The iron in all these is calculated as being in the metallic state, although in many of them it was much oxidized: a small part of the sulphur in some of the trials was present as sulphuric acid. When we take into consideration the several amounts of sulphur, we observe what appears very anomalous, that there is less sulphur at the end of six hours than after twelve. It may be asked, where the sulphur is gone, whence comes it again? In all our experiments this intermitting action of the sulphur is exhibited. It is probably connected with the increase of volume of the sublimed sulphur and sulphurous acid. A sudden evolu-

tion of these fumes will cause an immediate decrease in the quantity of sulphur in the portion of ore then tested; the gases above the ore being principally composed of sublimed sulphur, the admission of cold air, which always follows a rapid evolution of gas, probably the result of a reacting condensation, causes a portion of the sulphur to be deposited and absorbed again by the ore, so that the next quantity tried contains more sulphur. Regularity in the draught and heat of a calciner ought to be strictly attended to.

Another trial was made with an ordinary mixture of ores, every door of the calciner being clayed up air-tight, and allowing no air to be admitted except through the bridge under the fire, by which it was partially heated. A sample of ore taken out every hour for six hours and tested for sulphur gave as follows :—

	In the ore.	1st hour.	2nd hour.	3rd hour.	4th hour.	5th hour.	6th hour.
Sulphur ...	16·6	14·7	12·6	9·5	14	8·5	9·6

Here we have the same intermitting action after a few hours.

In the next experiment the calcination was continued as long as sulphur was present; or, as expressed practically, the ore was calcined *dead*. The quantity of sulphur was determined every four hours; the ore was turned over every two hours; the samples were taken out as the stirring commenced, and always from one part of the hearth.

	In the ore.	4 hours.	8 hours.	12 hours.	16 hours.	20 hours.	24 hours.	28 hours.	32 hours.	36 hours.	40 hours.	44 hours.
Sulphur	25·9	24·8	20·7	14·7	12·0	9·3	7·5	4·3	2·8	2·4	0·5	0·3
Given off every 4 hours ...		1·1	4·1	6·0	2·7	2·9	1·8	3·2	1·5	0·4	1·9	0·2

The intermittent action is not so fully developed in four-hour trials; but looking at the rate of loss each four hours, there is no doubt that it existed. It must be remembered in judging of the rate at which the sulphur passes off from the ore, that in the above and all calcinations the heat of the furnace is gradually increasing; showing the strong affinity the last portions of sulphur have for the metal.

In the above experiment the quantity of sulphates formed was also tested; the following are the results :—

After 4 hours a little sulphuric acid, no copper or iron in solution.
After 8 hours a little more acid, no metals.
After 12 hours both sulphate of copper and iron.
After 16 hours a mere trace of acid.

After 20 hours a half per cent. of copper as sulphate, no iron.

After 24 hours nearly the same.

After 28 hours a mere trace of acid and copper, no iron.

After 32 hours a mere trace of acid and copper, no iron.

After 36 hours trace of acid, no metal.

After 40 hours no trace of acid or metal.

After 44 hours no trace of acid or metal.

Several other experiments were tried of a like kind with similar results, to which we may have recur to in connexion with others of a different description in order to arrive at the causes of these reactions in a calciner.

For the purpose of saving time and fuel a compound calciner has been tried, and we believe is still in use in some works where one bed or hearth is placed over the other. The ore is put into the top bed, then passes through holes in the floor to the second or middle, and then to the third or lower bed, where it receives the highest heat; so that nine hours' calcination is equal to three hours in each bed. The first experiments made with this sort of calciner had for their object to ascertain the length of time required to calcine dead. The ore was tested for sulphur every four hours, allowing sixteen hours in each bed; each sample was also boiled in water, and the sulphuric acid determined.

	Ore.	1st bed.				2nd bed.					3rd bed.				
		4 hours.	8 hours.	12 hours.	16 hours.	20 hours.	24 hours.	28 hours.	32 hours.	36 hours.	40 hours.	44 hours.	48 hours.	52 hours.	56 hours.
Sulphur..	31	30·5	29·4 Iron	14·8 Iron	11·8 Copper Iron	22·9	18·1	21·0 Copper	15·6 Copper	8 Copper	3·0	4·5	2·8	1·5	0·5
In solution ... Sulphuric acid ...		·3	·7	2·3	8·4	·3	·4	·7	1·0	1	·3	·4	·4	·0	·0

The two following tables contain the results of experiments made with ores calcined only for a short time, the whole ingredients of the ore being taken in testing.

Table I.—A mixture of ores not analysed before being put into the furnace.

	1st bed.				2nd bed.				3rd bed.		
	1 hour.	2 hours.	3 hours.	4 hours.	5 hours.	6 hours.	7 hours.	8 hours.	9 hours.	10 hours.	11 hours.
Copper......	12·4	12·0	12·4	12·4	12·1	12·0	12·5	11·3	11·7	13·5	13·2
Iron	26·3	26·3	25·5	25·2	24·0	24·9	25·0	25·2	27·5	27·5	27·5
Silica	31·9	30·8	26·5	29·1	30·6	28·6	30·8	28·3	30·5	31·6	33·2
Sulphur ...	26·8	28·3	34·4	30·4	28·1	28·7	27·4	28·9	24·0	14·7	18·4
Sulphuric acid ...	·2	·2	·1		·1	trace	trace	trace	·8	·5	·4	·1

Table II.—Cuba ore not analysed before being put into the furnace.

	1st bed.				2nd bed.				3rd bed.			
	1 hour.	2 hours.	3 hours.	4 hours.	5 hours.	6 hours.	7 hours.	8 hours.	9 hours.	10 hours.	11 hours.	12 hours.
Copper......	16·0	15·0	11·3	12·0	16·0	15·0	12·5	12·4	14·0	12·4	16·5	16·7
Iron.........	24·9	25·5	24·6	26·6	27·4	26·0	28·0	28·0	29·7	30·8	27·3	24·5
Silica	28·0	26·7	34·4	29·2	29·2	26·7	27·9	28·9	28·4	29·5	33·2	30·5
Sulphur ...	25·6	30·9	25·4	28·3	26·4	24·9	23·7	29·6	27·0	26·4	18·8	20·3
Sulphuric acid ...	·1	·3	·6	·6	·7	1·7	·9	1	·3	·5	·4	·7

These results speak for themselves. By casting the eye along the sulphur line, it will be observed that there is generally an increase of sulphur in the top beds, showing evidently that the air passing over them had been charged with sulphur; and where it is not cooled so as to deposit its sulphur, no change is effected excepting the heating of the ore.

To have a fair comparison, a single bed and a three-bedded calciner were charged with the same quality of ore, and each charge kept in the same length of time, the heat, stirring and other circumstances being kept, as near as possibly could be, the same. The quantity of sulphur only was estimated. The ore put in contained 26·5 per cent. sulphur.

Sulphur.	1 hour.	2 hours.	3 hours.	4 hours.	5 hours.	6 hours.	7 hours.	8 hours.	9 hours.	10 hours.	11 hours.	12 hours.	Loss in all.
3 beds ...	26·2	29·6	29·9	29·3	27·2	26·8	25·5	29 2	25·2	20·5	18·4	19·2	7·3
Single ...	28·3	23·6	18·6	29·2	24·4	12·2	18·1	20·0	15·9	18·8	17·5	16·2	10·3

These numbers speak in favour of a single bed calciner. One great drawback to the compound furnace is the regulating of the fire; this must be done to suit the ore upon the lowest bed, which is often not suitable to the charges in the upper beds, and consequently absorption of sulphur takes place. Trials have been made with three-bedded calciners, open at each end, the fireplace being enlarged; the fire plays over each bed, and the sulphur fumes are all carried away on the opposite end by the stack or culvert without passing over another bed, but with no other result than an increased consumption of fuel without any corresponding advantage.

We shall have an opportunity of entering more fully into the principles and reactions of the calcining process when treating of the calcination of the mat or coarse metal, which, being free from earthy matters, and more definite in composition, the action

will be more plainly developed. In the meantime we may refer to one or two other plans that have been adopted or tried in the calcination of ores.

Some smelters have ore-calciners on the top of the ore fusing-furnace, the roof of the furnace being the lowest bed of the calciner arranged in the same manner as we have described in the case of the three-bedded calciner. There being two beds for the calcining operations above, the ore is put into a hopper on the roof and let into the top bed; it is then passed by a hole through the floor into the second bed, and from thence in the same way to the fusing-furnace. Thus the ore is being calcined during the time taken for fusing two charges; by this means the waste heat of the fusing-furnace is taken advantage of to calcine the ore. At first sight this mode may appear to be very œconomical; but it is subject to certain practical drawbacks, such as a greater liability in the furnace to get out of repair, thereby stopping two operations: the mat obtained is neither so good nor rich in copper. From causes to be noticed as we proceed, these have prevented the adoption of this method in the trade.

Calciners have also been tried alongside of a fusing-furnace, so fitted that the waste heat of the furnace passed over them as through a flue; also causing the fire from the fusing-furnace to pass along a series of flues covered by fire-tiles forming the bottom of close-chambers, in which the ore was put to calcine, separate chambers being connected to condense the sulphur; but all schemes depending upon the employment of the waste heat of the fusing-furnaces have been found ultimately more costly than the ordinary calciner of one bed. The close-chamber for calcining the ores by heating them from under the floor we have seen tried in a variety of forms, but always with one result. While the plates forming the floor were at a red heat sufficient to cake the ore in contact with it, the surface of the ore was comparatively cold, never at the temperature necessary to volatilize the sulphur, so that the sulphur volatilized at the bottom was condensed on the surface. To prove that some of these experiments were not made on a niggardly scale, or without an anxiety to succeed, we may state that a patent furnace fitted up for this object under the eye of the patentee measured 54 feet in length and 9 in breadth, took 30,000 common and 30,000 fire-bricks, 100 fire-tiles, 20 tons fire-clay, 12 tons cast and 2 tons wrought iron, besides the lime and labour. The results of several weeks' trial, were one ton of ore, half-calcined in 54 hours, without obtaining an ounce of sublimed sulphur. In many of the plans proposed there is a want of knowledge of principles. Sulphur will not sublime freely from ores in an atmosphere of sulphur; there must be some means of drawing off the atmosphere of sulphur whenever formed to give

room for another, and so forth. This law must be attended to in all subliming operations to be successful.

Several plans have been proposed for conducting the fumes of the ordinary calciner into chambers or other apparatus, in order to collect the sulphur, arsenic, &c. that may sublime during the process of calcination. The long culverts which are generally in use effect this object to a great extent; and to render these more effective, some smelters have apparatus for causing water to be constantly trickling down them, so that the sulphur and metallic fumes passing through the water are mostly all condensed. Many tons' weight is deposited in the ordinary culverts during the year, which is collected from time to time, and all that is valuable extracted. A quantity of stuff from a culvert leading from ore calcining furnaces alone gave by analysis—

Silica	15·5
Peroxide of iron . .	7·5
Oxide of zinc . . .	5·5
Oxide of copper . .	·7
Sulphuric acid . .	16·4
Sulphur	1·4
Arsenious acid . .	31·8
Lime	·6
Antimony	·5
Water	20·0
	99·9

The water was not originally in combination, but had been added to prevent dust.

Many other plans have been proposed and patented for the saving of the sulphur that is at present allowed to escape into the atmosphere. Some purpose converting it into sulphuric acid, as a marketable material in connexion with the calcination; others to roast the ore in kilns, like those for burning lime, and condense the fumes, &c.: all have their merits as inventions. Any process of this sort to be useful must not interfere with the time of obtaining the copper, it being so much more valuable than any other ingredient in the ore. And no process can be adopted by the smelters that will take away all the sulphur, without at the same time requiring a new method of smelting; for according to the present method, a certain amount of sulphur is essential to the purifying and obtaining the metal. Some of the plans might do well to work by themselves at the mines, taking care to leave as much sulphur in the ore as would form a coarse metal of 30 per cent. copper after fusion. The greater portion of the arsenic, and an average of half the sulphur the ores contain, might thus be

collected, and the remaining ore would bring a much higher price than it does at present.

Other methods have been tried with success, so far as the fact of capability goes, to arrange the furnace, the draught, the heat, and the mixture of ores, so as to convert the copper into the state of a soluble sulphate ; the ore is afterwards boiled in water, to which, after passing from the ore, is added metallic iron, which precipitates the copper in the metallic state ; but no method has yet surpassed in point of œconomy the ordinary single bed calciner. However, we must not forget the fact, that at least 30,000 tons of sulphur of the value of about £200,000 pass into the atmosphere every year in the compass of a few miles, which somewhat reflects upon our character as practical men desirous of turning all things to account.

[To be continued.]

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 393.]

May 27 "ANALYTICAL Researches connected with Steiner's 1852.   Extension of Malfatti's Problem." By Arthur Cayley, M.A., Fellow of Trinity College, Cambridge.   Communicated by J. J. Sylvester, Esq., F.R.S.

The problem, in a triangle to describe three circles each of them touching the two others and also two sides of the triangle, has been termed after the Italian geometer, by whom it was proposed and solved, Malfatti's Problem.   The problem to which the author refers as Steiner's extension of Malfatti's Problem, is as follows : " To determine three sections of a surface of the second order, each of them touching the two others, and also two of three given sections of the surface of the second order," a problem proposed in Steiner's memoir ' Einige Geometrische Betrachtungen,' Crelle, t. i.   The geometrical construction of the problem in question is readily deduced from that given in the memoir just mentioned for a somewhat less general problem, viz. that in which the surface of the second order is replaced by a sphere ; it is for the sake of the analytical developments to which the problem gives rise that the author proposes to resume here the discussion of the problem.   The following is an analysis of the present memoir :—

§ 1. Contains a lemma which appears to the author to constitute the foundation of the analytical theory of the sections of a surface of the second order.

§ 2. Contains a statement of the geometrical construction of Steiner's extension of Malfatti's problem.

§ 3. Is a verification, founded on a particular choice of co-ordinates, of the construction in question.

§ 4. In this section, referring the surface of the second order to absolutely general co-ordinates, and after an incidental solution of the problem to determine a section touching three given sections, the author obtains the equations for the solution of Steiner's extension of Malfatti's problem.

§ 5. Contains a separate discussion of a system of equations, including as a particular case the equations obtained in the preceding section.

§§ 6 & 7. Contain the application of the formulæ for the general system to the equations in § 4, and the development and completion of the solution.

§ 8. Is an extension of some preceding formulæ to quadratic functions of any number of variables.

"On the Tides, Bed and Coasts of the North Sea or German Ocean." By John Murray, Esq. Communicated by George Rennie, Esq., F.R.S.

The author commences his paper by remarking that great similarity of outline pervades the western shores of Ireland, Scotland and Norway, and then observes that the great Atlantic flood-tide wave, having traversed the shores of the former countries, strikes with great fury the Norwegian coast between the Lafoden Isles and Stadland, one portion proceeding to the north, while the other is deflected to the south, which last has scooped out along the coast, as far as the Sleeve at the mouth of the Baltic, a long channel from 100 to 200 fathoms in depth, almost close in shore, and varying from 50 to 100 miles in width. After describing his method of contouring and colouring the Admiralty chart of the North Sea, he traces the course of the tide-wave among the Orkney and Shetland Islands along the eastern shores of Scotland and England to the Straits of Dover, and along the western shores of Norway, Denmark and the Netherlands, to the same point. He then remarks that the detritus arising from the continued wasting away of nearly the whole line of the eastern coasts of Scotland and England, caused by the action of the flood-tide, is carried by it, and at the present day finds a resting-place in the North Sea; and that this filling process is increased by the sand, shingle, and other matter brought through the Straits of Dover by the other branch of the Atlantic flood-tide. Hence, he remarks, the gradual shoaling of this sea, and the formation of its numerous sand-banks; the silting up the mouths of the Rhine, the Meuse, and the Scheldt; the formation of the numerous islands on the coast of Holland, that country itself, and much of Belgium; the deposits at the mouth of the Baltic, the islands in the Cattegat, and indeed the whole country of Sleswig, Denmark and Jutland.

The author then takes a view of the tides, and their effects upon the Baltic and its shores before the course of the tide-wave was checked by these shoals and low lands. He considers that, previous to these great changes, the flood-tide entering the North Sea between Norway and Scotland, would make directly towards the German coast, and necessarily heap up the waters in the Baltic considerably

above their present level, and that a great part of Finland, Russia, and Prussia bordering upon that sea, would thus every twelve hours be under water, in the same way as the waters now rise in the Bay of Fundy, at Chepstow, and other places, much above their ordinary level in the open sea; that the current outward, on the receding of the tide which these accumulated waters would occasion, combined with the rivers which fall into the Baltic, when checked by the following flood-tide, would cause deposits in the form of a bar tailing towards Sweden; and that an increase to these deposits would form shoals, drifts and islands, and eventually a long sandbank in outline, like the country of Denmark. He further considers that the tide being by these means prevented from entering the Baltic, may account for the subsidence of the waters of the Gulf of Bothnia better than can the upheaval of the northern part of Scandinavia.

The author then remarks that the great shoal of the North Sea is the Dogger Bank, and that its peculiar form is produced by the meeting of the cotidal waves, of which he traces the course. After bearing testimony to the value of the Admiralty chart of the southern portion of the North Sea, made under the direction of the late Captain Hewitt, he reverts to the importance of contouring such maps, in order to obtain something like a correct notion of the bottom of the sea; and in conclusion expresses a hope that the Admiralty will be induced to continue the survey of the North Sea, so well begun by Captain Hewitt.

June 10.—The following papers were read:—

" On the Structure and Development of Bone." By John Tomes, F.R.S., Surgeon Dentist to the Middlesex Hospital, and Campbell De Morgan, Surgeon to the Middlesex Hospital.

In this communication, the authors, after having briefly noticed the intimate structure of perfect bone as commonly recognised, proceed to the description of certain points connected with its structure and development, which they believe to have been hitherto entirely overlooked or only partially recognised.

These points have been arranged under the following heads:—

1. The Haversian and other canals of bone.
2. The laminæ of bone.
3. The lacunæ.
4. Haversian systems.
5. Ossified cartilage of joints.
6. Ossified cells.
7. Bone tissue.
8. Development of bone in temporary cartilage.
9. Growth of bone.

1. *Haversian and other canals of bone.*—Besides the Haversian canals, the authors have pointed out that there are found in bone sections spaces of an entirely different character, irregular in shape, and with an irregular festooned margin. Their margins correspond in outline with those of one or more Haversian systems, and precede

in many instances the formation of those systems. These spaces, produced by absorption, are called by the authors *Haversian spaces*. Unlike the Haversian canals which are surrounded by their own laminæ, these spaces are bounded by parts of several systems which have been encroached on by the process of absorption.

In examining various sections, or different parts of the same section, many of these spaces will be found which have become partially or entirely occupied by Haversian systems. They are found in the bone of subjects of all ages. The fact of removal of old tissue and replacement by new, which has been hitherto only assumed, is thus demonstrated.

2. *Laminæ of bone.*—Lamination is shown to be a constant character of mammalian bone; each lamina, when highly developed, is found to consist of a dark granular, and of a transparent part. The external margin of the outermost lamina of each Haversian system is irregularly indented and corresponds with the outline of a pre-existing Haversian space; while its internal margin and all the succeeding laminæ are regular in outline.

The laminæ are found as a general rule to surround their canal, which is usually placed in the centre of them. But sometimes the canals are eccentric, in which case either the laminæ on one side, though still surrounding the canal, are broader, or more are developed on one side than on the other. The lamina next to the perfected Haversian canal however is always complete, and is often composed of a transparent structureless tissue, like that which encircles the Haversian canals of the stag's antler at the time of shedding.

The presence of interstitial laminæ is readily accounted for; they are in fact the remains of pre-existing Haversian systems, or circumferential laminæ, parts of which have been removed by absorption.

The circumferential laminæ are not so constantly present as is generally described, and seldom entirely surround the shaft of a long bone. When present, they seem to indicate that the bone is nearly stationary in its growth. They are frequently intersected by numerous Haversian spaces and systems, so as at length to assume the characters of interstitial laminæ.

3. *Lacunæ.*—In young bone the lacunæ are more abundant, larger, and have more numerous canaliculi; in older bone they may exist without canaliculi, or the canaliculi and great part of the lacunæ themselves may be filled up with solid matter, so as to leave only a small space in the centre of the latter. The lacunæ and canaliculi are shown to have distinct walls.

In the circumferential laminæ are frequently found elongated tubes which the authors regard as modifications of lacunæ ; they run obliquely across the laminæ, generally in bundles. They frequently form communications with the canaliculi. In transverse section they are seen to have proper walls.

4. *Haversian systems.*—The authors have here pointed out that the anastomosis of the canaliculi of adjoining systems is rare in newly-developed systems, but is very common in those of greater age. It

has been seen too that it not unfrequently happens that a series of Haversian systems is contained within a common series of surrounding laminæ. Sometimes the Haversian systems are rendered quite solid by the narrowing of the Haversian canal and ultimate development of a mere lacuna in the centre of the system. The more recently developed Haversian systems, which occupy the Haversian spaces before described, are seen to be darker in colour than the older ones, from the greater abundance of canaliculi, and the more general granularity of the tissue.

5. *Ossified articular cartilage.*—This structure the authors have found in all the joints which they have examined, in the lower jaw, amongst others, where Kölliker failed to detect it. Towards the bone the tissue becomes in general granular and of a brownish colour and usually there is a distinct line of demarcation between the bone and the ossified cartilage; but sometimes they graduate insensibly the one into the other.

Towards the articular surface the margin is even and regular; but towards the bone it is deeply indented, from the bone advancing into it by rounded projections. Hence the articular cartilage varies in thickness. The authors believe that this, so far from being an indication of imperfect development, is in reality an evidence of design, and intended to give an uniform and unyielding surface for the cartilage to rest upon.

6. *Ossified cells.*—In the bones of aged people it is frequently observed that they become light and spongy, and after maceration contain a white powder in the cancellated structure. This powder the authors have found to be composed mainly of ossified nucleated cells, either detached or held together in masses. They are spherical, and contain a dark granular nucleus, which is surrounded by a thick transparent wall.

If portions of the cancelli be examined, they will be found to have similar cells adherent to their surfaces, or to those of the Haversian canals, with here and there canaliculi of adjoining lacunæ shooting into them, while the nuclei have themselves assumed the form of lacunæ. Similar cells may be found imbedded in parts of most sections of bone. In order to see this condition clearly, it is desirable that the sections and the loose cells should be mounted in Canada balsam.

7. *Bone tissue.*—The views generally entertained with regard to the ultimate structure of bone tissue are, the older one, that it consists of an aggregation of granules in a transparent matrix; and that which has been more recently put forward by Dr. Sharpey, that in many cases it is composed of ossified decussating fibres.

The authors have satisfied themselves that the ultimate structure of bone tissue is composed of minute granules or granular bodies imbedded in a clear or subgranular matrix; and that the appearance of fibres is due in many cases to the mode of illumination. By transmitted light passing through them in the long axis of the microscope the preparations show a granular or a structureless appearance, or alternations of a granular and structureless part. But under

an oblique light passing from one side only an appearance of minute flat fibres presents itself. This takes place even in the isolated cells of old bone, or in developing young bone. This appearance is most marked over the lacunæ and canaliculi. But if a part which thus appears fibrous be viewed under a light passing obliquely from all sides, as is effected by a Gillett's achromatic condenser, the fibres disappear, and we see only a granular appearance, with some tendency to arrangement in the granules. The fibrous appearance is in fact due to the shadows cast from the less transparent parts when the light passes obliquely, just as in the navicula the dots are replaced by lines. In thin sections torn from bone which has been macerated in acid, a reticulated appearance, similar to that figured by Dr. Sharpey, may be seen, only however when the object is slightly out of focus, or the light oblique and from one side. By careful adjustment of the object-glass and of the illuminating apparatus, this appearance may be shown to depend on the presence of the canaliculi.

8 and 9. *Development and growth of bone.*—The early condition of cartilage, and the changes which take place in it and in the cartilage cell before ossification, are particularly described; and also the mode by which they multiply and arrange themselves by segmentation, so that a long column or cluster of cells represents an original cell, the walls of which have coalesced with the surrounding hyaline tissue. The cells at the same time enlarge individually as they approach the point where ossification is going on, encroaching on the hyaline substance so as in many cases only to leave a fine line of intercolumnar tissue, or even to cause it to disappear altogether. The nucleus at the same time enlarges considerably, while the cell wall becomes thickened internally, until in the end it reaches the nucleus, which then becomes imbedded in firm tissue. Other changes now take place: either several cells are thrown into one cavity by the absorption of their contiguous walls, leaving the nuclei free in the common cavity; or the nucleus continues to occupy its parent cell, and sends off small processes, which extend outwards to the cell wall. At this stage the nucleus may be sometimes detached with the processes entire, but generally it is adherent, and may be seen to have become a lacuna with a central cavity and canaliculi; in addition to which a nucleus may be seen to occupy its interior; it has in fact become a nucleated cell, designated by the authors "*granular cell.*" The entire cell may now be detached from the intercolumnar tissue in which it lies.

The granular condition of the intercolumnar tissue and of the cell itself often renders the observation of this stage very difficult; but in rickety bone it is very readily shown, as in this disease there is a tendency for the cells to assume their permanent form before the deposit of bone-earth in any considerable quantity. To cells thus composed of an outer thickened cell wall and an inner granular cell (the cartilage nucleus of authors) which contains within it a nucleus (the nucleolus of writers), which stands in the relation of a nucleus to the future lacuna, the authors have given the name of "*lacunal cells,*" while the term granular cell has been applied to that which

is usually designated the nucleus. In transverse sections of bone immediately below the line of ossification, the lacunal, cells may be seen presenting different characters under different circumstances. Where two cells come into contact, the processes or canaliculi may be seen extending across from one to the other; but where the cell is surrounded by intercolumnar tissue, the processes are short and do not extend beyond the walls of their own cell; or if cells join at one point while the remainder is invested with intercolumnar tissue, the canaliculi will anastomose at the point of junction; while elsewhere they are few, short, and do not extend beyond the cell.

In the further process of development the cells and intercolumnar tissue become fused together so as no longer to be recognised as distinct parts; and the granular cell appears as a perfect lacuna with a large cavity and numerous large canaliculi. To bone in this condition the term primary bone has been applied. It speedily however undergoes a change preparatory to the formation of the more permanent secondary bone. Here and there in the line of ossification portions are removed by absorption, the spaces left being filled with small somewhat granular cells, lying in a transparent blastema, and through the agency of which the absorption has been in all probability effected. It would appear as though the cells grew at the expense of the surrounding tissue. These spaces correspond entirely to the Haversian spaces before described; and in them the secondary bone is in the first instance formed. The process of formation of secondary bone appears to be everywhere essentially the same, whether in the absorbed spaces, or on the surfaces, or in the membranes of the fœtal cranium, except that in the two latter cases there is a pre-existing fibrous tissue, which, before ossification begins, undergoes a change similar to that which occurs in the bone itself and is converted into a cellular mass. So that at the border where ossification is advancing there is only an arrangement of cells; while a little beyond that point the cells have fibrous tissue abundantly mixed up with them; and there is in fact a resemblance to fibrous tissue in an early state of formation. The formation of perfect bone is effected by means of cells, perhaps identical with those which are found replacing the previous tissue, but at all events undistinguishable from them by any microscopical characters. To these cells, which take part in the formation of bone, the authors have given the name of *" osteal cells* *."* In the case of laminated bone they arrange themselves side by side, and, together with the transparent blastema in which they lie, become impregnated with ossific matter, and permanently fused with the bone tissue with which they lie in contact. By the linear arrangement of these osteal cells lamination is produced. In the case of non-laminated bone the cells are simply os-

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* The views here brought forward of the removal and replacement of tissue through the agency of cells are, so far as the authors know, entirely new; and may have an important bearing on many points of physiology and pathology. Indeed, this is perhaps the first time that the fact (which has been generally assumed) of the entire absorption of tissue in the processes of nutrition, and its replacement by new tissue, has been demonstrated.

sified without arrangement. Lying amongst the osteal cells will be seen some which have accumulated around them a quantity of tissue which forms a thick investment to them ; they then become granular, and take on in every respect the characters of a lacunal cell. These are found deposited at intervals along the line of ossification and becoming blended with the general mass ; the granular cell remaining as a lacuna, and sending out processes amongst the cells in all directions. In old bone the cell character is in great part lost by a general blending of the constituents, but may in many specimens be still here and there recognised. Many instances are given in support of the conclusion that absorption of bone and of dental tissue is effected directly through the influence of cells, but these are necessarily excluded from this abstract ; indeed it is impossible to give any other than a very imperfect account of the contents of the paper within the prescribed limits, especially as the numerous illustrations which accompany the paper cannot be made use of.

" On Rubian and its Products of Decomposition. Part II. Action of Alkalies and Alkaline Earths on Rubian." By Edward Schunck, Esq., F.R.S.

From the author's experiments it appears that rubian is decomposed by the fixed alkalies, and by lime and baryta, but not by ammonia. The products of decomposition formed by the action of the alkalies and alkaline earths are five in number. They are as follows :—1st, *Alizarine* ; 2nd, *Verantine* ; 3rd, *Rubiretine* ; 4th, *Sugar* ; and 5th, a new substance, which the author denominates *Rubiadine.* The first four possess the same properties and composition as when formed by the action of acids on rubian. The fifth substance, rubiadine, occupies the place of rubianine, which it closely resembles. It crystallizes from an alcoholic solution in small yellow or orange-coloured needles. It is insoluble in boiling water, and when carefully heated it may be almost entirely volatilized, forming a sublimate of yellow micaceous scales, endowed with considerable lustre. By these two properties it may be distinguished from rubianine, which is soluble in boiling water, and cannot be heated without being decomposed. Its other properties coincide almost entirely with those of rubianine. Its composition is expressed by the formula $C_{32} H_{12} O_8$, and presuming that the formula for rubianine be $C_{32} H_{19} O_{15}$, it would differ from the latter only by the elements of 7 equivs. of water. Besides these substances, there is also formed a small quantity of a dark brown powder, which is soluble in alkalies, but insoluble in water and alcohol. This substance has precisely the same composition as the ulmic acid of Mulder, formed by the action of strong acids on cane-sugar. Its formation is doubtless due to the further action of the alkali on the sugar formed in the first instance.

Action of Ferments on Rubian.—It has long been suspected by chemists, that the colouring matter of madder owes its formation to some process of fermentation, but the exact nature of the process has hitherto remained unknown. That some process of decomposition takes place on extracting madder with cold tepid water and exposing the extract to a moderate temperature, is proved by the

fact that the extract, if concentrated, becomes after some time thick and gelatinous; and that the process of decomposition takes effect chiefly on the rubian is apparent, since the extract, after it has become gelatinous, is found to have lost its bitter taste and the greater part of its yellow colour.

In order to prepare the peculiar fermentative substance of madder, which has the power of effecting the decomposition of rubian, it is merely necessary to add to an extract of madder made with cold or tepid water, about an equal volume of alcohol. This causes the separation of a quantity of dark reddish-brown flocks, which are collected on a filter and washed with cold alcohol, until the percolating liquid, which is at first strongly coloured, becomes almost colourless. The substance on the filter has the appearance of a dark reddish-brown granular mass, which possesses in an eminent degree the power of decomposing rubian. It is a true ferment, to which the author gives the name of *Erythrozym*. If a quantity of it be added to a solution of rubian, and the mixture be left to stand at the ordinary temperature, a complete change is found to have taken place in the course of a few hours. The liquid is converted into a trembling jelly of a light brown colour, which is perfectly tasteless, insoluble in cold water, and so consistent, that if the solution of rubian was tolerably concentrated, the vessel may be turned over without its falling out. During this process none of the usual signs of fermentation are manifested. The liquid remains perfectly neutral, and no gas of any kind is disengaged. On treating the gelatinous mass resulting from the process with cold water, an almost colourless liquid is obtained, which contains the same species of sugar as that formed by the action of acids or alkalies on rubian. The mass left undissolved by the cold water consists partly of the ferment employed and partly of the substances formed by the decomposition of the rubian. These substances are six in number, of which three are bodies previously described, and three are new. They are,— 1st, *Alizarine*; 2nd, *Verantine*; 3rd, *Rubiretine*; 4th, a substance closely resembling rubiacine, which the author calls *Rubiafine*; 5th, a substance very similar to rubianine and rubiadine, on which he bestows the name of *Rubiagine*; and 6th, a peculiar fatty substance which he denominates *Rubiadipine*.

The three latter bodies, which are products peculiar to this process of decomposition, have the following properties :—

Rubiafine is obtained by crystallization from alcohol in yellow glittering plates and needles, which are sometimes arranged in starshaped or plume-shaped masses. It cannot be distinguished by any of its properties from rubiacine; its composition is however different. Like the latter it is converted by the action of persalts of iron into rubiacic acid. The author has again submitted rubiacine and rubiacic acid, together with some of their compounds, to analysis, and from a comparison of their composition with that of rubiafine, he infers that the formula of the latter is $C_{32} H_{13} O_9$, that of rubiacine $C_{32} H_{11} O_{10}$, that of rubiacic acid $C_{32} H_9 O_{17}$; and he inclines to the belief, that by the action of persalts of iron rubiafine first passes into

rubiacine, before being converted into rubiacic acid; while by the action of reducing agents rubiacic acid is reconverted, first into rubiacine, and then into rubiafine.

Rubiagine is never obtained in well-defined crystals. When its alcoholic solution is evaporated spontaneously, it is left behind in the shape of small lemon-yellow spherical grains, which when crushed and examined under a lens, are found to consist of small crystalline needles grouped round a centre. When heated it melts, and is decomposed without being volatilized. It is quite insoluble in boiling water. It is soluble in boiling nitric acid, with a yellow colour, and crystallizes out again on the solution cooling in shining needles. Its alkaline solutions are blood-red. The alcoholic solution gives on the addition of acetate of lead at first no precipitate, but the colour of the solution becomes dark yellow, and after some time, provided the solution be not too dilute, an orange-coloured granular precipitate subsides, which is the lead compound of rubiagine. If no deposit is formed, then the addition of water causes an orange-coloured flocculent precipitate, which after being washed with water, in order to remove the excess of acetate of lead, is found to be very little soluble in boiling alcohol, but is easily soluble in a boiling alcoholic solution of acetate of lead with a dark yellow or orange-colour. When rubiagine is treated with a boiling solution of perchloride of iron, it dissolves slightly, but is not converted into rubiacic acid. Rubiagine is distinguished from rubianine by its insolubility in water; from rubiadine, for which it might most easily be mistaken, by its being incapable of sublimation; and from rubiafine by its not being convertible into rubiacic acid. Its behaviour towards acetate of lead, which is different from that of all the other three substances, also serves to characterize it. The most probable formula for rubiagine is $C_{32} H_{14} O_{10}$, from which it appears that it differs from the substances just named merely by the elements of water. Its formation from rubian, like that of the substances allied to it, indicates the simultaneous formation of sugar.

Rubiadipine is a body of a fatty nature, as its name indicates. In its appearance and general properties it resembles rubiretine. It differs from the latter in always remaining soft and viscid, and never becoming hard and brittle, however long it may be heated. Its colour is yellowish-brown. When heated in a tube it emits acrid fumes, similar to those produced by fat when exposed to destructive distillation. When thrown into boiling water it melts and forms oily drops, which rise to the surface. Its alcoholic solution gives with acetate of lead a pale reddish-brown precipitate, soluble in an excess of the precipitant. An analysis of the lead compound conducted to the formula $C_{39} H_{24} O_5 + PbO$. If this formula be accepted as the true one, the author confesses his inability to explain the formation of rubiadipine from rubian. The great excess of hydrogen contained in it shows that some substance must be formed simultaneously containing a large proportion of oxygen, which has hitherto escaped detection.

Having examined generally the action of erythrozym on rubian,

the author next proposed to himself to inquire, by what means this action is either destroyed, retarded, or promoted, and whether any means exist of so modifying the action as to lead to the formation of particular substances in preference to others. From a variety of experiments undertaken with this object, he draws the following conclusions :—

1. There exist no means short of the complete destruction of the ferment, capable of arresting its action on rubian, except exposing it while in a moist state to the temperature of boiling water. Even when exposed to that temperature, after having been previously dried, its fermenting power is not entirely lost, but merely weakened.

2. By the addition of various substances, usually classed as antiseptic, such as sulphuric acid, arsenious acid, sugar of lead, corrosive sublimate, alcohol, and oil of turpentine, during the process of fermentation, the action of the ferment is not destroyed ; it is merely retarded and modified.

3. The more the action of the ferment on rubian is retarded, the more rubiretine and verantine, and the less alizarine are formed ; so much so, that in some cases the alizarine disappears entirely from among the products of decomposition, which then consist almost solely of rubiretine and verantine. The formation of rubiafine and rubiagine is promoted when the action of the ferment is moderately retarded, but diminishes again or entirely ceases when the retardation is very great. Of the two the rubiagine is the first to disappear when any retardation takes place.

4. By the addition of small quantities of alkalies during the process of fermentation, the action is, as regards its duration, if not promoted, at all events not retarded ; and as regards the relative quantities of the various substances produced, the amount of alizarine is thereby decidedly increased, while that of the rubiretine and verantine is diminished.

Most of the ordinary fermentative substances, such as albumen, cascine, gelatine, and yeast, are incapable of effecting the decomposition of rubian, even when mixtures of these various substances with watery solutions of rubian are allowed to stand until they enter into a state of putrefaction. Emulsine is the only substance capable of forming an effective substitute for erythrozym. Its action is similar to that of the latter substance ; it gives rise however to the formation of a much larger proportional quantity of alizarine. The action of the peculiar albuminous substance, discovered by Braconnot in the root of the *Helianthus tuberosus*, on rubian, was also examined. It exerts only a slight effect on the latter, the only products of decomposition formed being rubiretine and verantine.

The author considers the fact of erythrozym being almost the only ferment which is capable of effecting in any considerable degree the decomposition of rubian, as the best proof of its being a peculiar and distinct substance. When prepared by precipitation with alcohol, erythrozym is obtained as a chocolate-coloured granular mass. When dried it coheres into hard lumps, which are almost

black, and with difficulty reduced to powder. When the dry substance is heated on platinum foil, it emits a smell somewhere between that of burning peat and burning horn, and then burns without much flame, leaving a considerable quantity of residue, which on being further heated is soon converted into a white or grey ash.

After having once been precipitated from its watery solution, even by alcohol, erythrozym cannot again be dissolved in water. If it be mixed while in a moist state with water, it forms a reddish-brown turbid liquid, in which it exists however merely in a state of suspension. Erythrozym is not an uncombined substance, but is a definite compound of an organic body with lime. When treated with acids the lime is removed, and the colour of the substance changes from reddish-brown to yellowish-brown. If a mixture of erythrozym and water be allowed to stand for some time, the former enters into a state of putrefaction, accompanied by a disengagement of gas. After it has entered on this stage of decomposition, it loses in great part the power of decomposing rubian, but acquires that of producing an acid reaction in a solution of sugar. Erythrozym contains nitrogen, but in much smaller proportion than most other fermentative substances. Its composition, when in a freshly precipitated state, is expressed by the formula $C_{56} H_{34} N_2 O_{40} + 4CaO$. When allowed to decompose, it loses carbonic acid, water, and lime. A quantity which had been employed for the purpose of effecting the decomposition of rubian, and then separated again from the products of the action, had a composition corresponding to the formula $C_{52} H_{32} N_2 O_{30} + 3CaO$.

In conclusion, the author gives the results of some experiments undertaken with the view of ascertaining whether madder contains more than one colouring matter or not. He infers from his experiments, that the purpurine of other chemists is not a substance of determinate composition; that it consists sometimes of alizarine alone, sometimes of verantine alone, sometimes of a variable mixture of both; that only one colouring matter, viz. alizarine, can be obtained from madder; that purpurine, madder-purple, and the various similar bodies derived from madder, owe their property as colouring matters to an admixture of alizarine; and that they are simply the latter substance in a state of impurity.

LXXV. *Intelligence and Miscellaneous Articles.*

NEW METHOD OF PRECIPITATING OXIDE OF TIN AND SEPARATING IT FROM OTHER BODIES, AND OF COMBINING IT WITH SILK, WOOLLEN AND COTTON FABRICS. BY J. LÖWENTHAL.

WHILE endeavouring to find a simple and accurate means of detecting small quantities of perchloride of tin in protochloride, the author found that alkaline sulphates possess the property of decomposing the perchloride and precipitating hydrated oxide from the aqueous solution, while they do not exercise the slightest influence upon the protochloride when atmospheric air is excluded.

In order to prove that it was the alkaline sulphate and not the

water dissolving it which effected the decomposition, Löwenthal made the following experiments.

A gramme of anhydrous perchloride of tin was treated in two experiments with 100 and 60 cubic cent. of cold water. The one solution began to lose its clearness after 2 or 3 hours, the latter not till after 36 hours, then very feebly, and it was not milky until after a week. Having thus ascertained the proportion of water which might be employed without fearing decomposition, he mixed,—

a. 0·5 grm. perchloride with a solution of 1 grm. crystallized sulphate of soda in 30 cub. cent. water at 64° F. A precipitate was immediately formed.

b. The experiment *a.* was altered by adding 0·125 grm. hydrochloric acid, 1·12 sp. gr., to the perchloride of tin. The liquid was at first turbid, and after one minute the decomposition was complete.

c. On using 0·25 grm. hydrochloric acid, it was not complete until after 3 minutes, with 0·375 grm. 10 min., with 0·5 grm. 43 min., and with 0·75 grm. of acid the liquid remained clear for several hours.

d. 0·5 grm. perchloride with 0·5 grm. hydrochloric acid and 30 cub. cent. water containing 2·5 grms. crystallized sulphate of soda, were mixed at 64° F.: remained at first clear, and the decomposition was not completed until after 7 minutes.

e. The experiment *c.* with 0·5 grm. acid repeated. The liquid at first clear, immediately gave a precipitate when heated to about 108° F.

f. 0·5 grm. protochloride treated with only 15 grms. water containing 1 grm. sulphate of soda. Liquid at first clear, gave a precipitate when heated which did not disappear on cooling.

g. Metastannic acid long digested with hydrochloric acid and the residue dissolved in water. On the addition of sulphate of soda the decomposition ensued immediately. It follows from these data that,—

1. The decomposition takes place in the cold only when a certain quantity of water is present.

2. In the absence of free acid, with an adequate quantity of water and with about 2 equivs. sulphate of soda to 1 equiv. perchloride of tin, the decomposition takes place immediately.

3. Free acid hinders the decomposition more or less in proportion to its quantity. Its influence may be removed by the careful addition of ammonia. Chloride of ammonium does not cause the decomposition of the perchloride.

4. With equal quantities of free acid the decomposition takes place more rapidly, the greater the quantity of sulphate of soda.

5. Heat favours the decomposition in all cases.

Löwenthal likewise found that the sulphates of magnesia, alumina, protoxide of manganese, iron, zinc and copper, peroxide of iron, produced the same decomposition; moreover, nitrates of soda, ammonia, baryta, zinc and copper.

He found that the precipitate was pure hydrated oxide of tin, and never contained any of the acid of the precipitant. He considers the decomposition to be represented by

$$SnCl^2 + 4NaOSO^3 + 4HO = SnO^2 \, 2HO \, (?) + 2NaCl$$
$$+ 2(NaO, HO, 2SO^3).$$
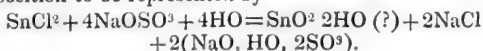
Although in this equation 4 equivs. sulphate of soda are given, the decomposition takes place with 2 and even with 1 equiv., but a larger quantity of water is then necessary. In this case acid is set free; however, with a sufficiency of water this does not permanently hinder the decomposition (in which the tendency of the hydrated oxide of tin to separate certainly plays an important part), especially at an elevated temperature. The influence of this latter condition was indeed to be expected, since Fremy has shown that the solution of hydrated oxide of tin in dilute sulphuric acid is decomposed by boiling.

It now remained to prove that by this means all the tin present as perchloride was precipitated, and that the separation of the tin and chlorine was complete. The decomposition was effected with sulphate of soda (A) and nitrate of ammonia (B). 100 parts of a solution of perchloride of tin gave the following results:—

	A.			B.	
	Tin.	Chlorine.		Tin.	Chlorine.
1.	4·964	5·976	1.	4·935	5·962
2.	4·975	5·956	2.	4·948	5·971

Consequently 100 parts anhydrous perchloride of tin would contain—

	Calculated.	Found.			
		A.		B.	
		1.	2.	1.	2.
Sn eqt. wt.	45·30	45·37	45·51	45·38	45·32
2Cl eqt. wt.	54·70	54·63	54·49	54·62	54·68
	100·00	100·00	100·00	100·00	100·00

These results show that the separation is effected with great accuracy.

This behaviour of oxide of tin admits of several useful applications; among others—

1. The detection of tin in almost any liquid.

2. It affords an exact and simple means of quantitatively determining tin, as well as an easy and perfect means of separating it from chlorine and other halogens, from alkalies, &c.

It is of especial worth to manufacturers in determining the commercial value of tin salts. In the absence of delicate balances they generally have recourse to the nitric acid test, which always gives an incorrect result, in consequence of the volatilization of the chloride of tin.

In some experiments on the quantitative separation of oxide from binoxide of tin, I obtained on mixing 9·0723 of the above solution to which a few drops of hydrochloric acid had been added with 10·6196 of a solution of protochloride corresponding to 16 per cent. of protoxide, 0·6792 protoxide of tin corresponding to 0·5339 grm., or calculated for 100 parts of the solution 5·002, consequently only 0·042 per cent. more than the average of the above results, which amounts to 4·960.

3. It affords a very suitable means of combining oxide of tin with cotton, woollen and silk stuffs in dyeing. Löwenstein considers that the experiments made by him justify the opinion that this method,

properly employed*, has great advantages over the use of the expensive alkaline stannates.

4. Dark colours containing tin may be conveniently made by this means.—*Journ. für Prakt. Chem.* lvi. p. 366.

REMARK ON ART. LVIII. (OF PHIL. MAG. FOR NOVEMBER) BY MR. SYLVESTER, "THAT THE NON-EXISTENCE OF REAL ROOTS IN ANALYTIC GEOMETRY CORRESPONDS TO THE *REDUCTIO AD ABSURDUM* OF EUCLID."

All imaginary results are expressible by $a + b\sqrt{-1} + f - c\sqrt{-1}$; and if $c = b$, we have a real result. Hence if we try the case of one of two presumed equal quantities exceeding the other, this is equivalent to taking $b > c$; if less, to $b < c$; in both cases an imaginary result would follow. But if the algebraic treatment resulted in $b = c$, we should necessarily find $\sqrt{-1}$ disappear, and the presumed equality would really exist as $a + f$. This being the geometric process generally followed in the *reductio ad absurdum,* may perhaps throw some light on the verification of Mr. Sylvester's important suggestion.

23 Walpole Street, Chelsea,   S. M. DRACH.
  November 22, 1852.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1852.

Chiswick.—October 1. Fine. 2. Uniformly overcast: very fine: clear. 3. Clear: very fine: clear: slight rain. 4. Constant and very heavy rain: clear. 5. Clear and boisterous. 6. Rain: clear at night. 7. Cloudy: fine: uniformly overcast. 8. Overcast: fine. 9. Clear: very fine: rain. 10. Overcast. 11, 12. Very fine. 13. Fine: dusky haze. 14. Foggy: uniform haze: at night clear above, hazy near the horizon. 15, 16. Foggy: cold haze: clear above at night. 17. Dense fog. 18. Very fine throughout. 19. Foggy: very fine: dense fog at night. 20. Foggy: exceedingly fine: hazy. 21. Foggy: hazy: rain. 22. Hazy: rain. 23. Cloudy: rain. 24. Cloudy: very fine: heavy rain at night. 25. Heavy rain: clear at night. 26. Slight fog: rain. 27. Rain. 28. Densely overcast: cloudy: clear. 29. Clear: overcast: rain. 30. Overcast: rain. 31. Clear and fine: bright sun: overcast at night.

              Mean temperature of the month .... 46°·38
              Mean temperature of Oct. 1851 .... 51 ·25
              Mean temperature of Oct. for the last twenty-six years ... 50 ·50
              Average amount of rain in Oct. .... 2·60 inches.

Boston.—Oct. 1, 2. Fine. 3. Cloudy: rain early A.M. 4, 5. Cloudy: rain A.M. and P.M. 6. Cloudy: rain P.M. 7. Fine: rain early A.M. 8, 9. Fine. 10. Cloudy. 11—13. Fine. 14. Foggy. 15—17. Cloudy. 18. Fine. 19. Cloudy. 20. Fine. 21, 22. Rain: rain A.M. 23. Cloudy: rain A.M. 24, 25. Fine. 26. Fine: rain P.M. 27. Fine: rain A.M. and P.M. 28. Rain: rain A.M. 29, 30. Cloudy. 31. Fine: rain early A.M.

Sandwick Manse, Orkney.—Oct. 1. Clear: hoar-frost: fine: hoar-frost. 2. Rain. 3. Bright: showers. 4. Cloudy. 5. Cloudy: clear. 6, 7. Sleet-showers. 8. Sleet-showers: cloudy. 9. Drizzle: cloudy. 10. Showers. 11, 12. Bright: cloudy. 13. Rain: cloudy. 14. Cloudy: fine. 15. Bright: clear: fine. 16. Fine: hoar-frost: hazy: fine. 17. Fine: cloudy: rain. 18. Rain: cloudy. 19. Drizzle. 20. Cloudy: drizzle. 21. Fine: cloudy: fine. 22. Hazy: drizzle. 23. Cloudy: clear: fine. 24. Clear: fine: aurora. 25. Clear: fine. 26, 27. Showers. 28. Showers: bright: clear. 29. Cloudy. 30. Showers. 31. Rain: cloudy.

              Mean temperature of Oct. for twenty-five previous years ...... 47°·55
              Mean temperature of this month .... 46 ·88
              Average quantity of rain in Oct. for six years .... 4·39 inches.

                          * The mode of employment is not stated by the author.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1852. Oct.	Barometer					Thermometer					Wind			Rain		
	Chiswick Max.	Chiswick Min.	Boston 8¼ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney, Sandwick 9½ a.m.	Orkney, Sandwick 8½ p.m.	Chiswick 1 p.m.	Boston	Orkney, Sandwick	Chiswick	Boston	Orkney, Sandwick
1.	29.540	29.436	29.07	29.24	29.39	55	45	45.5	46	41½	sw.	wsw.	calm	.02	……	.08
2.	29.536	29.342	28.93	29.33	29.37	59	41	43	48	49	sw.	wsw.	n.	.03	……	.18
3.	29.849	29.690	29.27	29.50	29.69	59	33	52	51½	48	w.	w.	nne.	……	.05	.68
4.	29.752	29.001	29.36	29.62	29.38	56	49	46	50	47½	s.	s.	ene.	1.06	……	.12
5.	29.486	28.987	28.45	29.18	29.32	56	39	53	48	44	w.	w.	n.	.10	.81	……
6.	29.741	29.547	29.12	29.44	29.40	52	39	47	44	39	nw.	wnw.	nw.	.06	.04	.03
7.	29.924	29.780	29.47	29.72	29.80	53	37	41	42	37	n.	wnw.	nw.	……	.10	.22
8.	29.969	29.892	29.48	29.87	29.94	47	28	40	44	41	n.	wnw.	nw.	……	……	.08
9.	29.969	29.961	29.58	29.81	29.91	49	31	37.5	45	46	ne.	nw.	calm	.05	……	.10
10.	29.982	29.919	29.68	29.84	29.91	55	38	48	50	49	n.	w.	nnw.	……	……	.15
11.	30.226	30.052	29.96	29.91	29.84	57	38	45.5	48½	49	ne.	n.	wnw.	……	……	.11
12.	30.365	30.331	30.00	30.16	30.26	56	32	44	51½	49	ne.	nnw.	wnw.	……	……	.04
13.	30.360	30.303	30.00	30.30	30.34	54	41	40	51	51	e.	nne.	sw.	……	……	.18
14.	30.291	30.251	29.89	30.30	30.28	48	46	48	49	49½	e.	calm	ssw.	……	……	……
15.	30.258	30.135	29.83	30.35	30.35	54	44	53	44	42½	ne.	ne.	calm	……	……	……
16.	30.195	30.124	29.76	30.30	30.33	52	36	48	51½	48	e.	sc.	sse.	……	……	……
17.	30.234	30.175	29.80	30.19	30.16	54	31	43	52	50	ne.	w.	w.	……	……	……
18.	30.345	30.305	29.97	30.00	30.23	56	30	44	52½	53½	ne.	nnw.	nw.	……	……	……
19.	30.451	30.151	29.97	30.00	30.05	55	33	43	52½	50	n.	calm	ese.	.26	.10	.53
20.	30.357	30.006	29.85	29.95	29.90	59	49	46	48	49	s.	w.	se.	.14	.10	.14
21.	30.006	29.867	29.60	29.94	29.83	59	50	54	52	46	s.	sw.	se.	.18	.07	.18
22.	29.762	29.642	29.33	29.60	29.42	60	50	56	51½	43	s.	s.	c.	.65	……	.19
23.	29.686	29.652	29.20	29.50	29.44	58	37	47	48	44	sw.	sw.	s.	.55	……	.02
24.	29.629	29.590	29.10	29.28	29.31	57	31	42	48½	43	sw.	s.	se.	.33	……	.14
25.	29.446	29.285	29.05	29.44	29.35	50	30	38	43½	39½	w.	s.	e.	.06	……	……
26.	29.377	28.877	29.00	29.66	29.53	49	41	36	44½	40½	s.	w.	ne.	.02	……	.16
27.	29.195	29.842	28.66	30.00	29.86	46	40	46	41	46	nw.	n.	e.	.22	.27	.07
28.	29.644	29.318	29.02	29.98	29.86	49	40	42.5	40½	46½	nw.	nw.	e.	.14	.43	.23
29.	29.742	29.463	29.38	29.69	29.60	50	48	40	44½		sw.	nw.	ese.		.10	.13
30.	29.631	29.595	29.20	29.36	29.48	59	48	48	48		sw.	nw.	se.			.08
31.	29.813	29.585	29.12			61	44	53			w.	w.			.34	.28
Mean.	29.896	29.752	29.41	29.784	29.808	54.32	38.45	46.0	47.77	46.00				3.87	2.41	4.12

LXXVI. *On the Thermal Effects experienced by Air in rushing through small Apertures.* By J. P. JOULE and W. THOMSON[*].

THE hypothesis that the heat evolved from air compressed and kept at a constant temperature is mechanically equivalent to the work spent in effecting the compression, assumed by Mayer as the foundation for an estimate of the numerical relation between quantities of heat and mechanical work, and adopted by Holtzmann, Clausius, and other writers, was made the subject of an experimental research by Mr. Joule[†], and verified as at least approximately true for air at ordinary atmospheric temperatures. A theoretical investigation, founded on a conclusion of Carnot's[‡], which requires no modification[§] in the dynamical theory of heat, also leads to a verification of Mayer's hypothesis within limits of accuracy as close as those which can be attributed to Mr. Joule's experimental tests. But the same investigation establishes the conclusion, that that hypothesis cannot be rigorously true except for one definite temperature within the range of Regnault's experiments on the pressure and latent heat of saturated aqueous vapour, unless the density of the vapour both differs considerably at the temperature 100° Cent. from what it is usually supposed to be, and for other temperatures and pressures presents great discrepancies from the gaseous laws. No experiments, however, which have yet been published on the density of saturated aqueous vapour are of sufficient accuracy to admit of an unconditional statement of the indications of theory regarding the truth of Mayer's hypothesis, which cannot therefore be considered to have been hitherto sufficiently

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* Communicated by the Authors; having been read to the British Association at Belfast, Sept. 3, 1852.
† Phil. Mag. May 1845, p. 375, "On the Changes of Temperature produced by the Rarefaction and Condensation of Air."
‡ Transactions of the Royal Society of Edinburgh (April 1849), vol. xvi. part 5, "Appendix to Account of Carnot's Theory," §§ 46–51.
§ Trans. Royal Soc. Edinb. (March 1851) vol. xx. part 2; or Phil. Mag. S. 4. vol. iv. "On the Dynamical Theory of Heat," § 30.

tested either experimentally or theoretically. The experiments described in the present communication were commenced by the authors jointly in Manchester last May. The results which have been already obtained, although they appear to establish beyond doubt a very considerable discrepancy from Mayer's hypothesis for temperatures from $40°$ to $170°$ Fahr., are far from satisfactory; but as the authors are convinced that, without apparatus on a much larger scale, and a much more ample source of mechanical work than has hitherto been available to them, they could not get as complete and accurate results as are to be desired, they think it right at present to publish an account of the progress they have made in the inquiry.

The following brief statement of the proposed method, and the principles on which it is founded, is drawn from §§ 77, 78 of Part IV. of the series of articles on the Dynamical Theory of Heat republished in this Magazine from the Transactions of the Royal Society of Edinburgh in 1851* (vol. xx. part 2. pp. 296, 297).

Let air be forced continuously and as uniformly as possible, by means of a forcing-pump, through a long tube, open to the atmosphere at the far end, and nearly stopped in one place so as to leave, for a short space, only an extremely narrow passage, on each side of which, and in every other part of the tube, the passage is comparatively very wide; and let us suppose, first, that the air in rushing through the narrow passage is not allowed to gain any heat from, nor (if it had any tendency to do so) to part with any to, the surrounding matter. Then, if Mayer's hypothesis were true, the air after leaving the narrow passage would have exactly the same temperature as it had before reaching it. If, on the contrary, the air experiences either a cooling or a heating effect in the circumstances, we may infer that the heat produced by the fluid friction in the rapids, or, which is the same, the thermal equivalent of the work done by the air in expanding from its state of high pressure on one side of the narrow passage to the state of atmospheric pressure which it has after passing the rapids, is in one case less, and in the other more, than sufficient to compensate the cold due to the expansion; and the hypothesis in question would be disproved.

The apparatus consisted principally of a forcing-pump of $10\frac{1}{2}$ inches stroke and $1\frac{3}{6}$ internal diameter, worked by a hand-lever, and adapted to pump air, through a strong copper vessel† of 136 cubic inches capacity, (used for the purpose of

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* See also Dynamical Theory of Heat, part 5. Trans. Roy. Soc. Edinb. 1852.

† This and the forcing-pump are parts of the apparatus used by Mr. Joule in his original experiments on air. See Phil. Mag. S. 3. vol. xxvi. p. 370 (1845).

equalizing the pressure of the air,) into one end of a spiral leaden pipe 24 feet long and $\frac{5}{16}$ths of an inch in diameter, provided with a stopcock at its other end. The spiral was in all the experiments kept immersed in a large water-bath.

In the first series of experiments, the temperature of the bath was kept as nearly as possible the same as that of the surrounding atmosphere; and the stop-cock, which was kept just above the surface of the water, had a vulcanized india-rubber tube tied to its mouth. The forcing-pump was worked uniformly, and the stop-cock was kept so nearly closed as to sustain a pressure of from two to five atmospheres within the spiral. A thermometer placed in the vulcanized india-rubber tube, with its bulb near the stop-cock, always showed a somewhat lower temperature than another placed in the water-bath*; and it was concluded that the air had experienced a cooling effect in passing through the stop-cock.

To diminish the effects which might be anticipated from the conduction of heat through the solid matter round the narrow passage, a strong vulcanized india-rubber tube, a few inches long, and of considerably less diameter than the former, was tied on the mouth of the stop-cock in place of that one which was removed, and tied over the mouth of the narrower. The stop-cock was now kept wide open, and the narrow passage was obtained by squeezing the double india-rubber tube by means of a pair of wooden pincers applied to compress the inner tube very near its end, through the other surrounding it. The two thermometers were placed, one, as before, in the bath, and the other in the wide india-rubber tube, with its bulb let down so as to be close to the end of the narrower one within. It was still found that, the forcing-pump being worked as before, when the pincers were applied so as to keep up a steady pressure of two atmospheres or more in the spiral, the thermometer placed in the current of air flowing from the narrow passage showed a lower temperature than that of the air in the spiral, as shown by the other. Some-

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* When the forcing-pump is worked so as to keep up a uniform pressure in the spiral, and the water of the bath is stirred so as to be at a uniform temperature throughout, this temperature will be, with almost perfect accuracy, the temperature of the air as it approaches the stop-cock. It is to be remarked, however, that when, by altering the aperture of the stop-cock, or the rate of working the pump, the pressure within the spiral is altered, even although not very suddenly, the air throughout the spiral, up to the narrow passage, alters in temperature on account of the expansion or condensation which it is experiencing, and there is an immediate corresponding alteration in the temperature of the stream of air flowing from *the rapids*, which produces often a most sensible effect on the thermometer in the issuing stream.

times the whole of the narrow india-rubber tube, the wooden pin-cers, and several inches of the wider tube containing the thermo-meter, were kept below the surface of the bath, and still the cooling effect was observed; and this even when hot water, at a tempe-rature of about 150° F., was used, although in this case the observed cooling effect was less than when the temperature of the bath was lower.

As it was considered possible that the cooling effects observed in these experiments might be due wholly or partly to the air reaching the thermometer-bulb before it had lost all the *vis viva* produced by the expansion in the narrow passage, and conse-quently before the full equivalent of heat had been produced by the friction, and as some influence (although this might be expected to diminish the cooling effect) must have been produced by the conduction of heat through the solid matter round the air, especially about the narrow passage, an attempt was made to determine the whole thermal effect by means of a calorime-trical apparatus applied externally. For this purpose the india-rubber tubes were removed, and the stop-cock was again had recourse to for producing the narrow passage. A piece of small block-tin tube, about 10 inches long, was attached to the mouth of the stop-cock, and was bent into a spiral, as close round the stop-cock as it could be conveniently arranged. A portion of the block-tin pipe was unbent from the principal spiral, and was bent down so as to allow the stop-cock to be removed from the water-bath, and to be immersed with the exit spiral in a small glass jar filled with water. The forcing-pump was now worked at a uniform rate, with the stop-cock nearly closed, for a quarter of an hour, and then nearly open for a quarter of an hour, and so on for several alternations. The temperatures of the water in the large bath and in the glass jar were observed at frequent stated intervals during these experiments; but, instead of there being any cooling effect discovered when the stop-cock was nearly closed, there was found to be a slight elevation of tem-perature during every period of the experiments, averaging nominally ·06525° F. for four periods of a quarter of an hour when the stop-cock was nearly closed, and ·06533° when it was wide open, or, within the limits of the accuracy of the observa-tions, ·065° in each case; a rise due, no doubt, to the rising temperature of the surrounding atmosphere during the series of experiments. Hence the results appear at first sight only nega-tive; but it is to be remarked that, the temperature of the bath having been on an average $3\frac{1}{2}$° F. lower than that of the water in the glass jar, the natural rise of temperature in the glass jar must have been somewhat checked by the air coming from the

principal spiral; and had there been no cooling effect due to rushing through the stop-cock when it was nearly closed, would have been more checked when the stop-cock was wide open than when it was nearly closed, as the same number of strokes of the pump must have sent considerably more air through the apparatus in one case than in the other. A cooling effect on the whole, due to the rushing through the nearly closed stop-cock, is thus indicated, if not satisfactorily proved.

Other calorimetric experiments were made with the stop-cock immersed in water in one glass jar, and the air from it, conducted by a vulcanized india-rubber tube, to flow through a small spiral of block-tin pipe immersed in a second glass jar of equal capacity; and it was found that the water in the jar round the stop-cock was cooled, while that in the other, containing the exit spiral, was heated, during the working of the pump, with the stop-cock nearly closed, and a pressure of about three atmospheres in the principal spiral. The explanation of this curious result is clearly, that the water round the stop-cock supplied a little heat to the air in the first part of the rapids, where it has been cooled by expansion and has not yet received all the heat of the friction, and that the heat so obtained, along with the heat produced by friction throughout the rapids, raises the temperature of the air a little above what it would have had if no heat had been gained from without; so that about the end of the rapids the air has a temperature a little above that of the surrounding water, and is led, under the protection of the india-rubber tube, to the exit spiral with a slightly elevated temperature. This is what would *necessarily* happen in any case of an arrangement such as that described, if Mayer's hypothesis were strictly true; but then the quantity of heat emitted to the water in the second glass jar, from the air in passing through the exit spiral, would be exactly equal to that taken by conduction through the stop-cock from the water in the first. In reality, according to the discrepancy from Mayer's hypothesis, which the other experiments described in this communication appear to establish, there must have been somewhat more heat taken in by conduction through the stop-cock than was emitted by it in flowing through the exit spiral; but the experiments were not of sufficient accuracy, and were affected by too many disturbing circumstances, to allow this difference to be tested.

To obtain a decisive test of the discrepancy from Mayer's hypothesis, indicated by the experiments which have been described, and to obtain either comparative or absolute determinations of its amount for different temperatures, some alterations in the apparatus, especially with regard to the narrow passage

and the thermometer for the temperature of the air flowing from it, were found to be necessary by Mr. Joule, who continued the research alone, and made the experiments described in what follows.

A piece of brass piping, *a* (see the accompanying sketch drawn half the actual size), was soldered to the termination of the leaden spiral, and a bit of calf-skin leather, *b*, having been tightly bound over its end, it was found that the natural pores of the leather were sufficient to allow of a uniform and conveniently rapid flow of air from the receiver. By protecting the end over which the leather diaphragm was bound with a piece of vulcanized india-rubber tube *c*, the former could be immersed to the depth of about two inches in the bath of water. A small
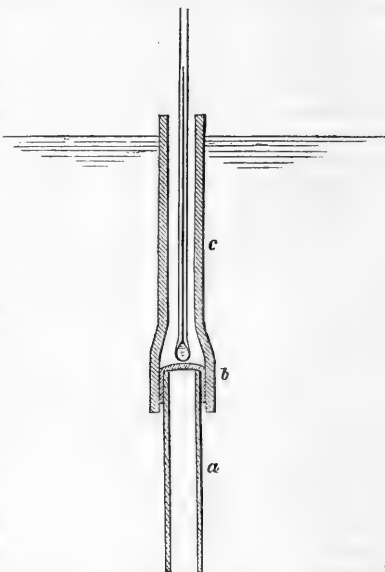
thermometer*, having a spherical bulb $\frac{1}{6}$th of an inch in diameter, was placed within the india-rubber tube, the bulb being allowed to rest on the central part of the leather diaphragm†.

In making the experiments, the pump was worked at a uniform rate until the pressure of the air in the spiral and the temperature of the thermometer had become sensibly constant. The water of the bath was at the same time constantly stirred, and by various devices kept as uniform as possible during each series of experiments. The temperature of the stream of air having been observed, the same thermometer was immediately plunged into the bath to ascertain its temperature, the difference between the two readings giving of course the cooling effect of the rushing air.

* We had two of these thermometers, one of which had Fahrenheit's, the other an arbitrary scale.

† The bulb was kept in this position for convenience sake, but it was ascertained that the effects were not perceptibly diminished when it was raised $\frac{1}{4}$ of an inch above the diaphragm.

According to theory*, the cooling effect for a given temperature would be independent of the kind of aperture and of the copiousness of the stream, and would be simply proportional to the logarithm of the pressure, if the insulation of the current against gain or loss of heat from the surrounding matter were perfect, and if the thermometer be so placed in the issuing stream as to be quite out of the *rapids*. On this account the values of the cooling effect divided by the logarithm of the pressure were calculated, and are shown in the last columns of the tables of results given below. When this was done for the first two series of experiments, the discrepancies (see columns 5 of the first two of the tables given below) were found to be so great, and, especially among the results of the different experiments for the higher temperature of 160° F., all made with the pressure and other circumstances as nearly as possibly the same, so irregular, that great uncertainty was felt as to the numerical results, which must obviously have been much affected by purely accidental circumstances. At the same time it was noticed, that in the case of Series 1, in which the temperature of the bath was always as nearly as possible that of the atmosphere, and different pressures were used, the discrepancies showed a somewhat regular tendency of the value of the cooling effect divided by the logarithm of the pressure to increase with ˙the pressure; which was probably owing to the circumstance that the stream was more copious, and that less of the cooling effect was lost (as some probably was in every case) by the conduction of heat from without, the higher the pressure under which the air approached the narrow passage. Hence in all the subsequent experiments the quantity of air pumped through per second was noted.

The following Tables show the results obtained from ten series of experiments conducted in the manner described :—

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* See Account of Carnot's Theory, Appendix II. Trans. Royal Soc. Edinb. vol. xvi. p. 566 ; and Dynamical Theory, § 75, Trans. Royal Soc. Edinb. vol. xx. pp. 296 ; or Phil. Mag. vol. iv. p. 431. The numbers shown in the table of § 51 of the former paper being used in the formula of § 75 of the latter, and 1390 being used for J, we find (according to the numerical data used formerly for deriving numerical results from the theory) how much heat would have to be added to each pound of the issuing stream of air to bring it back to the temperature it had when approaching the narrow passage ; and this number, divided by ˙24, the specific heat of air under constant pressure, would be the depression of temperature (in Centigrade degrees) actually experienced by the air when no heat is communicated to it in or after the rapids.

Series 1.

Col. 1. Quantity of air pumped in cubic inches per second. A.	Col. 2. Temperature of bath. T.	Col. 3. Pressure of air in atmospheres. P.	Col. 4. Cooling effect. D.	Col. 5.* Cooling effect divided by logarithm of pressure. $\dfrac{D}{\log P}$.
Not noted.	$\overset{\circ}{6}1$	1·79	$\overset{\circ}{0}\cdot5$	$\overset{\circ}{1}\cdot98$
Not noted.	61	2·64	0·9	2·13
Not noted.	61	2·9	0·7	1·51
Not noted.	61	3·22	1·5	2·95
Not noted.	61	3·4	1·4	2·64
Not noted.	61	3·61	1·4	2·51
Not noted.	61	3·61	1·3	2·33
Not noted.	61	3·61	1·4	2·51
Not noted.	61	3·84	1·5	2·57
Not noted.	61	4·11	1·7	2·77
Mean......	61	......	......	2·39

Series 2.

Not noted.	160	2·64	0·264	0·62
Not noted.	160	2·64	0·396	0·94
Not noted.	160	2·64	0·66	1·56
Not noted.	160	2·64	0·528	1·25
Not noted.	160	2·64	0·66	1·56
Mean......	160	2·64	0·502	1·18

Series 3.

5·6	170·8	3·61	0·396	0·71
5·6	170·8	4·11	0·528	0·86
5·6	170·8	4·11	0·66	1·08
5·6	170·8	4·11	0·726	1·18
5·6	170·8	4·26	0·66	1·05
8·4	170·8	4·78	0·858	1·26
8·4	170·8	4·98	0·858	1·23
Mean 6·4	170·8	4·28	0·67	1·05

Series 4.

5·6	37·8	3·4	0·8	1·51
5·6	38·8	3·4	1·1	2·07
5·6	37·9	3·61	0·6	1·08
5·6	44·4	3·04	1·1	2·28
5·6	45·3	3·04	0·9	1·86
5·6	46·3	3·04	1·0	2·07
Mean 5·6	41·75	......	......	1·81

* The true value of $\dfrac{D}{\log P}$ for any particular temperature would be the depression of temperature that would be experienced by air approaching the narrow passage at that temperature and under ten atmospheres of pressure, since P is measured in atmospheres, and the *common* logarithm is taken.

Table (*continued*).

Series 5.

Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.
Quantity of air pumped in cubic inchesper second.	Temperature of bath.	Pressure of air in atmospheres.	Cooling effect.	Cooling effect divided by logarithm of pressure.
A.	T.	P.	D.	$\dfrac{D}{\log P}$
8·4	46·8	3·84	1·2	2·06
8·4	38·7	4·11	1·8	2·93
8·4	39·3	4·11	1·8	2·93
Mean 8·4	41·6	......	......	2·64

Series 6.

11·2	39·7	4·4	1·7	2·64
11·2	40·9	4·4	1·9	2·95
11·2	41·9	4·4	1·5	2·33
11.2	43	4·4	1·5	2·33
Mean 11·2	41·38	4·4	1·65	2·56

Series 7.

1·4	64·1	1·9	0·3	1·08
1·4	64·2	1·87	0·45	1·65
1·4	64·0	1·9	0·4	1·43
1·4	64·2	1·9	0·5	1·79
1·4	64·3	1·9	0·45	1·61
Mean 1·4	64·16	1·894	0·42	1·51

Series 8.

2·8	64·2	2·41	0·5	1·31
2·8	64·3	2·41	0·5	1·31
2·8	64·5	2·41	0·5	1·31
2·8	64·7	2·41	0·7	1·83
2·8	64·7	2·41	0·6	1·57
Mean 2·8	64·48	2·41	0·56	1·46

Series 9.

5·6	64·6	2·9	0·8	1·73
5·6	64·7	2·9	0·8	1·73
5·6	64·8	3·04	0·8	1·66
5·6	65·0	2·97	0·7	1·48
Mean 5·6	64·775	......	......	1·65

Series 10.

11·2	65	4·11	1·2	1·95
11·2	65·1	4·11	1·3	2·12
11·2	65·1	4·11	1·4	2·28
Mean 11·2	65·06	4·11	1·3	2·12

The numbers in the last column of any one of these tables show, by their discrepancies, how much uncertainty there must be in the results on account of purely accidental circumstances.

The following table is arranged, with double argument of temperature and of quantity of air passing per second, to show a comparison of the means of the different series (Series 3 being divided into two, one consisting of the first five experiments, and the other of the remaining two).

Table of Mean Values of $\dfrac{D}{\log P}$ in different series of experiments.

		Quantity of air passing per second.				
		1·4	2·8	5·6	8·4	11·2
Temperature of bath.	41½°	......	......	1·81	2·64	2·56
	64½	1·51	1·46	1·65	......	2·12
	171	......	......	·98	1·25	......

The general increase of the numbers from left to right in this table shows that very much of the cooling effect must be lost on account of the insufficiency of the current of air. This loss might possibly be diminished by improving the thermal insulation of the current in and after the rapids; but it appears probable that it could be reduced sufficiently to admit of satisfactory observations being made, only by using a much more copious current of air than could be obtained with the apparatus hitherto employed.

The decrease of the numbers from the upper to the lower spaces, especially in the one complete vertical column (that under the argument 5·6), shows that the cooling effect is less to a remarkable degree for the higher than for the lower temperatures. Even from 41° to 65° F. the diminution is most sensible; and at 171° the cooling effect appears to be only about half as much as at 41°.

The best results for the different temperatures are probably those shown under the arguments 8·4 and 11·2, being those obtained from the most copious currents; but it is probable that they all fall considerably short of the true values of $\dfrac{D}{\log P}$ for the actual temperatures; and we may consider it as perfectly established by the experiments described above, that *there is a final cooling effect produced by air rushing through a small aperture at any temperature up to* 170° F., *and that the amount of this cooling effect*

decreases as the temperature is augmented. Now according to the theoretical views on this subject brought forward in the papers on "Carnot's Theory," and "On the Dynamical Theory of Heat," already referred to, a cooling effect was expected for low temperatures; and the amount of this effect was expected to be the *less* the *higher* the temperature; expectations which have therefore been perfectly confirmed by experiment. But since the excess of the heat of compression above the thermal equivalent of the work was, in the theoretical investigation, found to diminish to zero* as the temperature is raised to about 33° Cent., or 92° Fahr., and to be negative for all higher temperatures, a *heating* instead of a *cooling* effect would be found for such a temperature as 171° F., if the data regarding saturated steam used in obtaining numerical results from the theory were correct. All of these data except the *density* had been obtained from Regnault's very exact experimental determinations; and we may consequently consider it as nearly certain, that the true values of the density of saturated aqueous vapour differ considerably from those which were assumed. Thus, if the error is to be accounted for by the *density* alone, the fact of there being any cooling effect in the air experiments at 171° Fahr. (77° Cent.) shows that the density of saturated aqueous vapour at that temperature must be greater than it was assumed to be in the ratio of something more than 1416 to 1390, or must be more than 1·019 of what it was assumed to be: and, since the experiments render it almost if not absolutely certain, that even at 100° Cent. air rushing through a small aperture would produce a final cooling effect, it is probable that the density of steam at the ordinary boiling-point, instead of being about $\dfrac{1}{1693\cdot5}$, as it is generally supposed to be, must be something more than $\dfrac{1430\cdot6}{1390}$ of this; that is, must exceed $\dfrac{1}{1645}$.

With a view to ascertain what effect would be produced in the case of the air rushing violently against the thermometer-bulb, the leather diaphragm was now perforated with a fine needle, and the bulb placed on the orifice so as to cause the air to rush between the leather and the sides of the bulb. With this arrangement the following results were obtained:—

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* See the table in § 51 of the Account of Carnot's Theory, from which it appears that the element tabulated would have the value 1390, or that of the mechanical equivalent of the thermal unit, at about 33° Cent.

Series 11.

A.	T.	P.	D.	$\frac{D}{\log P}$
11·2	64̊	3·22	3̊·5	6̊·90
11·2	64	3·31	3·5	6·73
11·2	64	3·61	3·8	6·82
11·2	64	2·30	4·0	11·05
11·2	64	3·31	6·1	11·73
11·2	64	2·58	4·7	11·41
11·2	64	4·78	5·3	7·80
11·2	64	1·9	4·0	14·34
Mean 11·2	64	......	......	9·60

The great irregularities in the last column of the above table
are owing to the difficulty of keeping the bulb of the thermo-
meter in exactly the same place over the orifice.   The least varia-
tion would occasion an immediate and considerable change of
temperature; and when the bulb was removed to only $\frac{1}{4}$ of an
inch above the orifice, the cooling effects were reduced to the
amount observed when the natural pores alone of the leather
were employed.   There can be no doubt but that the reason
why the cooling effects experienced by the thermometer-bulb
were greater in these experiments than in the former is, that in
these it was exposed to the current of air in localities in which
a sensible portion of the mechanical effect of the work done by
the expansion had not been converted into heat by friction, but
still existed in the form of *vis viva* of fluid motion.   Hence this
series of experiments confirms the theoretical anticipations for-
merly published[*] regarding the condition of the air in *the rapids*
caused by flowing through a small aperture.

LXXVII. *On the Method of Symmetric Products.   By* JAMES
COCKLE, *M.A., of Trinity College, Cambridge; Barrister-at-
Law of the Middle Temple*[†].

1. THE conclusions of Abel and Sir W. R. Hamilton respect-
ing the impossibility of solving equations of the fifth
degree are rendered doubtful by recent investigations of Mr. G.
B. Jerrard.   New fields of research thus seem to open upon us.
My present object is to point out the general scope of the method
of symmetric products, and to offer some remarks which may
assist in the inquiry as to how far that method is calculated to
throw light upon the theory of equations of the higher degrees.

2. Any n symbols $v_1, v_2, . . , v_n$ may be considered as the roots of an equation of the form

$$v^n + s_1 v^{n-1} + s_2 v^{n-2} + . . + s_n = 0.$$

Let V_r be a linear and homogeneous function of these symbols and of the form indicated by

$$V_r = v_1 + \alpha_r v_2 + \beta_r v_3 + \gamma_r v_4 + . . + \epsilon_r v_{n-1} + \vartheta_r v_n.$$

Also let the product of m such functions be denoted by $\pi_m(v_n)$, so that

$$\pi_m(v_n) = V_1 V_2 V_3 . . V_{m-1} V_m.$$

Then, if $m = n-1$ and n be not greater than 4, we may so determine $\alpha, \beta, \ldots$ as to render the product $\pi_{n-1}(v_n)$ a symmetric function of v. The case in which $n = 2$ is scarcely an exception, for we have

$$v_1 - v_2 = \{(v_1 + v_2)^2 - 4v_1 v_2\}^{\frac{1}{2}},$$

and the anomalous function corresponding to the symmetric product is, in a manner, symmetric. I shall denote this last function by P'.

3. The m factors constituting a product may be regarded as the roots of

$$V^m + t_1 V^{m-1} + t_2 V^{m-2} + . . + t_m = 0.$$

If when the products are symmetric we make

$$\pi_2(v_3) = P_2, \quad \pi_3(v_4) = P_3,$$

the values of V which constitute the above two symmetric products are derived from the respective equations

$$V^2 - (3v_1 + s_1)V + P_2 = 0 ;$$
$$V^3 - (4v_1 + s_1)V^2 + (8v_1^2 + 4s_1 v_1 - s_1^2 + 4s_2)V - P_3 = 0 ;$$

in both of which cases

$$t_1 = nv_1 + s_1,$$

and t_1 and t_2 are functions in which v_1 is the only symbol that occurs unsymmetrically.

4. Let the result of the elimination of x, between

$$x^n + p_1 x^{n-1} + p_2 x^{n-2} + . . + p_n = 0,$$

and

$$y - f(x) = 0,$$

be represented by

$$y^n + q_1 y^{n-1} + q_2 y^{n-2} + . . + q_n = 0.$$

Then, when n is either 3 or 4, f is so constructed as to make $\pi_{n-1}(y_n)$ or P_n vanish. We are thus conducted to solutions of cubic and biquadratic equations. It is a sign of the generality

of the *method of symmetric products* that if we make

$$y = f(x) = (a + x)^{-1},$$

we arrive at the solution of a cubic which I gave at pp. 248, 249 of vol. ii. of the Cambridge Mathematical Journal. A sketch of the process by which this solution is shown to fall under the method will be found at pp. 228, 229 of the 52nd volume of the Mechanics' Magazine. In a note to my paper on the Method of Vanishing Groups published in the Cambridge and Dublin Mathematical Journal for May last, I developed the application of the method of symmetric products to the solution of a biquadratic. As my purpose is not to repeat but to endeavour to extend former results, I shall content myself with referring to the latter paper.

5. There is a species of symmetric function which I have called 'critical,' and considered in this Journal (S. 3. vol. xxviii. p. 191), and with great detail in the third and concluding volume of the Mathematician. Their characteristic property, and one that has an important bearing on the theory of equations, is that if to each of the quantities ($y_1, y_2, \ldots, y_n$ for example) symmetrically involved there be added the same quantity (b), the transformed function (of $y_1 + b, y_2 + b, \ldots$) is free from b, and does not differ in value from the original one. Let us represent a certain normal form of homogeneous critical function by $\phi_m(y_n)$, where, as above, m is the degree and n the order of the function; and let

$$[\phi'_2(y_2) = C'], \quad \phi_2(y_3) = C_2, \quad \phi_3(y_4) = C_3,$$

and in general

$$\phi_{n-1}(y_n) = C_n.$$

Then, if c', c_2 and c_3 be certain determinable constant multipliers, and, as in the preceding article, y be supposed to replace v in P, the following relations hold,

$$[P' = c'(C')^{\frac{1}{2}}], \quad P_2 = c_2 C_2, \quad P_3 = c_3 C_3.$$

6. When n is greater than 4, can we obtain the analogous relation

$$P_n = c_n C_n \, ?$$

Or, if not, and we have

$$\pi_{n-1}(y_n) = c_{n-1} C_{n-1} + R_{n-1}, \quad \ldots \quad (a)$$

what are the value and properties of R_{n-1}? Or, can we attain any available results by taking a value of m greater or less than $n-1$?

If R_{n-1} vanishes, the answer to the second of these questions will give an affirmative reply to the first. And, under any circumstances, there are considerations which seem to render such an assumption as (a) a desirable one.

7. Any proposed extension of the method of symmetric pro-

ducts to the higher prime equations will find a type in its application to those of the fifth degree; and the questions incidentally suggested in the latter case will equally arise, perhaps in a more general form, in the discussion of equations of the higher degrees. When the degree is composite, simplifications of the processes will probably be obtainable. But I shall here confine myself to quintic equations, in the theory of which the following questions now present themselves: (1) Is there a symmetric product? and (2), if not, does our search after one suggest an unsymmetric function with any peculiar properties?

8. Retaining the assumption $m = n - 1$, and continuing to replace v by y, let

$$\pi_4(y_5) = c_4 C_4 + R_4 = P_4 + U_4,$$

where P_4 is symmetric and U_4 evanescent or unsymmetric, and it remains to be seen whether R_4 is equal to U_4.

9. It is first to be remarked, that $f(x)$ may always be determined so as to reduce the equation in y to the form

$$y^5 + q_3 y^2 + q_5 = 0. \quad \cdots \quad (b)$$

And when this relation subsists P_4 becomes equal to zero. Hence, if a symmetric product exist, there will be no difficulty in making it vanish. If not, we may always assume that

$$\pi_4(y_5) = U_4.$$

For effecting the transformation (b) we may avail ourselves of Mr. Jerrard's process, or of the more convenient one which I have given for the purpose (Phil. Mag. S. 3. vol. xxxii. pp. 50, 51), and in which the solution of $\phi(x, y, z) = 0$ is supposed to be effected by the Method of Vanishing Groups.

10. The quantity P_4 may be expressed by

$$\Sigma \cdot y^4 + E\Sigma \cdot y_1{}^3 y_2 + F\Sigma \cdot y_1{}^2 y_2{}^2 + G\Sigma \cdot y_1{}^2 y_2 y_3 + H\Sigma \cdot y_1 y_2 y_3 y_4;$$

and, guided by the analogy afforded by the application of the method to biquadratics, I shall first proceed to inquire whether $\pi_4(y_5)$ can be made to take the above form. If not, our object must be to reduce the unsymmetric part U_4 within the narrowest possible limits. Whether the process which follows be the most advantageous for our purpose may be a subject of future and formal inquiry. But its strong *primâ facie* claims warrant its adoption here.

11. As well to fix our ideas as to facilitate our operations, let us write

$$Y_1 = y_1 + \alpha_1 y_2 + \beta_1 y_3 + \gamma_1 y_4 + \delta_1 y_5,$$
$$Y_2 = y_1 + \alpha_2 y_2 + \beta_2 y_3 + \gamma_2 y_4 + \delta_2 y_5,$$
$$Y_3 = y_1 + \alpha_3 y_2 + \beta_3 y_3 + \gamma_3 y_4 + \delta_3 y_5,$$
$$Y_4 = y_1 + \alpha_4 y_2 + \beta_4 y_3 + \gamma_4 y_4 + \delta_4 y_5,$$
$$\pi_4(y_5) = Y_1 Y_2 Y_3 Y_4.$$

12. The conditions requisite for the symmetry of the terms in y^4 are

$$1 = a_1 a_2 a_3 a_4 = \beta_1 \beta_2 \beta_3 \beta_4 = \gamma_1 \gamma_2 \gamma_3 \gamma_4 = \delta_1 \delta_2 \delta_3 \delta_4.$$

13. The conditions derived from the terms in $y_1^3 y_r$ and $y_1 y_r^3$ are

$$\left. \begin{array}{l} E = \Sigma . \alpha = \Sigma . \beta = \Sigma . \gamma = \Sigma . \delta = \\ \Sigma . \alpha_1 a_2 a_3 = \Sigma . \beta_1 \beta_2 \beta_3 = \Sigma . \gamma_1 \gamma_2 \gamma_3 = \Sigma . \delta_1 \delta_2 \delta_3 \end{array} \right\}.$$

14. Those derived from the terms in $y_1^2 y_r^2$ are

$$F = \Sigma . \alpha_1 a_2 = \Sigma . \beta_1 \beta_2 = \Sigma . \gamma_1 \gamma_2 = \Sigma . \delta_1 \delta_2.$$

15. Hence, each of the four expressions

$$(\alpha_1, \alpha_2, \alpha_3, \alpha_4), \quad (\beta_1, \beta_2, \beta_3, \beta_4), \quad (\gamma_1, \gamma_2, \gamma_3, \gamma_4), \quad (\delta_1, \delta_2, \delta_3, \delta_4)$$

involves the four roots of the equation

$$z^4 - Ez^3 + Fz^2 - Ez + 1 = 0 \quad . \quad . \quad . \quad . \quad (c)$$

in some (hypothetically) determinable but as yet undetermined order. That order must of course be excluded which renders the values of Y equal, for in such case we should be led to the relation

$$\pi_4(y_5) = q_1^4,$$

a nugatory result.

16. The equation (c) is recurring, and its roots are of the forms $\lambda, \lambda^{-1}, \mu, \mu^{-1}$. We are, consequently, at liberty to start with the assumptions

$$\alpha_1 = \lambda, \quad \alpha_2 = \lambda^{-1}, \quad \alpha_3 = \mu, \quad \alpha_4 = \mu^{-1}.$$

And here for the present at least I leave the discussion, with the remark that if no evanescent form of U_4 be discoverable, that function may possibly be found to possess the properties of a *modulus* of the given equation. A theory of conjugate equations may hence arise. If Abel's argument be undisputed, it is hard to conceive that the theory of equations should not admit of an extension analogous to that which he himself gave to the theory of elliptic integrals. If its validity be denied, we may pursue our present course with more sanguine anticipation. The consequences of supposing that U_n is equal to zero will be a subject for after inquiry.

2 Pump Court, Temple,
   November 1, 1852.

Postscript. I shall perhaps be forgiven for adding, that in the Philosophical Magazine for June 1843 (S. 3. vol. xxii. pp. 502, 503), I gave a solution of an imperfect cubic which is free from at least one defect under which that of Cardan labours,—the arbitrary character of the operation by which the indeterminate result of substitution is broken up into two separate equations. I have also there obtained roots in an unobjectionable form. The process is one of great simplicity.

LXXVIII. *Note on the Heat of Chemical Combination.*
By Dr. ANDREWS*.

IN the last Number of this Journal, I observe that Dr. Woods, referring to some observations of mine at the late meeting of the British Association in this place, states that I had previously *conjectured* one of the fundamental truths suggested by his theory, viz. "that decomposition produces as much cold as the combination of the elements produced heat," and afterwards claims "by right of prior publication" the *proof* of this law. Dr. Woods's first paper on this subject appeared in the Philosophical Magazine for October 1851. Now in a paper of mine on the Thermal Changes accompanying Basic Substitutions, which was published in the Philosophical Transactions for 1844, and for which the Council of the Royal Society awarded one of the Royal Medals, the following passage occurs (p. 32) :—

"In the preceding observations it has been assumed, that *if the union of two substances be attended with the evolution of a certain definite quantity of heat, their separation will be attended with the absorption of the same quantity of heat.* Although this proposition in the abstract is very probable, it requires to be demonstrated by direct experiment ; and it is the more important to do so, as it will furnish, if true, a means of verifying the accuracy of our results. The reactions now described enable us to test it by experiment in one particular set of cases. In fact, if we take three bases, such as potash, oxide of copper, and water, capable of displacing one another in the above order, and if we measure the changes of temperature produced when the first and second, first and third, and second and third bases displace one another, then the change of temperature arising from the first substitution should be equal to the difference between the changes of temperature produced by the two latter. A few examples will illustrate this point."

In a subsequent paper, which appeared in the Numbers of this Journal for May and June 1848, I revert to the same subject. After comparing the quantity of heat produced by the precipitation of metallic copper by zinc with the quantities produced by the combination of those metals with oxygen, and with that due to the substitution of oxide of zinc for oxide of copper, I proceed to remark "that this comparison assumes the truth of the principle (which I have in other inquiries endeavoured to illustrate, and is indeed almost self-evident), that when, in the course of any chemical reaction, the constituents of a compound are separated from one another, there is a quantity of heat thereby

absorbed equal to that which would have been evolved if the same substances had entered into combination."

It would, I conceive, be impossible to express a physical law in language more precise, or having less of the character of conjecture. I have not considered it necessary to extend this Note by quoting the experimental proofs, which will be found partly in the papers from which I have made these extracts, and partly in my other publications on the Heat of Combination.

Queen's College, Belfast,
    November 12, 1852.

LXXIX. *On the Electro-chemical Polarity of Gases.*
By W. R. Grove, *Esq., M.A., F.R.S.**

[With a Plate.]

THE different effect of electricity upon gases and liquids has long been a subject of interest to physical inquirers. There are, as far as I am aware, no experiments which show any analogy in the electrization of gases to those effects now commonly comprehended under the term electrolysis. Whether gases at all conduct electricity, properly speaking, or whether its transmission is not always by the disruptive discharge, the discharge by convection, or something closely analogous, is perhaps a doubtful question; but I feel strongly convinced that gases do not conduct in any similar manner to metals or electrolytes.

In a paper published in the year 1849[†], I have shown that hydrogen or atmospheric air intensely heated, showed no sign of conduction for voltaic electricity even when a battery of very high intensity was employed.

In the Eleventh, Twelfth, and Thirteenth Series of Faraday's Experimental Researches, the line of demarcation between induction across a dielectric and electrolytic discharge is repeatedly adverted to; induction is regarded as an action of contiguous particles, and as a state of polarization anterior to discharge, whether disruptive, as in the case of dielectrics, or electrolytic, as in electrolytes. See §§ 1164—1298—1345—1368, &c.

Mr. Gassiot, in a paper published in the year 1844[‡], has shown that the static effects, or effects of tension, produced by a voltaic battery, are in some direct ratio with the chemical energies of the substances of which the battery is composed; in other words, that in a voltaic series, whatever increases the decomposing power of the battery when the terminals are united by an electrolyte, also increases the effects of tension produced by it, when its terminals are separated by a dielectric.

* From the Philosophical Transactions for 1852, part i.; having been received by the Royal Society January 7, and read April 1, 1852.
    † Philosophical Transactions, 1849, p. 55.   ‡ Ibid. 1844, p. 39.

In none of the above papers, and in no researches on electricity of which I am aware, is there any experimental evidence that the polarization of the dielectric is or may be chemical in its nature, that, assuming a dielectric to consist of two substances having antagonist chemical relations, as for instance, oxygen and hydrogen, the particles of the oxygen would be determined in one direction, and those of the hydrogen in the other; the only experimental result bearing on this point with which I am acquainted, is the curious fact which was observed by Mr. Gassiot and some other electricians who experimented with him in the year 1838, viz. that when two wires forming the terminals of a powerful battery were placed across each other, and the voltaic arc taken between them, the extremity of the wire proceeding from the positive end of the battery was rendered incandescent, while the negative wire remained comparatively cool; it was at that time believed that there was some effect exhibited here *extra* the voltaic circuit.   Shortly afterwards I showed that with all, or at all events a great number of metals, the positive terminal was more heated than the negative, and that the portion of the crossed wire which was positive became more incandescent than that of the negative, from the greater heating effect developed at the positive point when the disruptive discharge took place.   I suggested as an explanation of this phænomenon, the possibility that in air, as in water, or other electrolyte, the oxygen or electro-negative element was determined to the positive terminal, and that from the union of the metal with that oxygen a greater heating effect was developed.   This, with some other impressions, I mentioned in a letter to my friend Dr. Schönbein, not intended for publication, but which shortly afterwards found its way into print*.

Though by no means thinking that this explanation was in every respect satisfactory, there were many arguments in its favour; and the fact strongly impressed my mind as evincing a very striking difference in character between the effect of the discharge at the positive and negative terminals, and as presenting, as far as it went, a distant analogy to the effect of electrolysis.

In the year 1848, while experimenting with Mr. Gassiot with a nitric acid battery consisting of 500 well insulated cells, I made the following experiment:—Two wires of platinum $\frac{1}{40}$th of an inch in diameter, forming the terminals of the battery, were immersed in distilled water; the negative wire was then gradually withdrawn until it reached a point a quarter of an inch distant from the surface of the water.   A cone of blue flame was now perceptible, the water forming its base, and the point of the

wire its apex; the wire rapidly fused, and became so brilliant that the cone of flame could be no longer perceived, and the globule of fused platinum was apparently suspended in air and hanging from the wire; it appeared sustained by a repulsive action, like a cork ball on a *jet d'eau*, and threw out scintillations in a direction away from the water. The surface of the water at the base of the cone was depressed, and divided into little concave cups, which were in a continual agitation. When the conditions were reversed and the negative wire immersed, the positive wire being at the surface, similar phænomena ensued, but not nearly in so marked a manner; the cone was smaller, and its base much more narrow in proportion to its height.

This experiment, the beautiful effect of which requires to be seen to be appreciated, indicates a new mode of transmission of electricity partaking of the electrolytic and disruptive discharges. Not possessing a battery of this enormous intensity, I have not been able to examine this phænomenon more in detail; but I have from time to time made many other experiments on the voltaic arc taken in various gaseous media, with the view of ascertaining the state of the intervening media anterior to, during, and after the discharge; these experiments have hitherto given me no results of any value. In the voltaic arc, the intense heat developed so affects the terminals and so masks the proper electrical effect, that the difficulty of isolating the latter is extreme; and I have latterly sought for some modified form of electric discharge which should be intermediate between the voltaic arc and the ordinary Franklinic discharge, or that from the prime conductor of a frictional machine; for something, in short, which should yield greater quantitative effects than the electrical machine, but not dissipate the terminals, as is done by the voltaic arc.

An apparatus, to which M. Despretz was kind enough to call my attention recently at Paris, seemed to promise me some aid in this respect. It was constructed by M. Ruhmkorff, on the ordinary plan for producing an induced current, viz. a coil of stout wire round a soft iron core, with a secondary coil of fine wire exterior to it, having an ingenious self-working contact breaker attached; from the attention paid to insulation in the construction of this apparatus, very exalted effects of induction could be procured. Thus in air rarefied by the air-pump, an aurora or discharge of 5 or 6 inches long could be obtained from the secondary coil, and in air of ordinary density a spark of one-eighth of an inch long.

I procured one of these apparatus from M. Ruhmkorff; the size of the coil portion of the apparatus is 6·5 inches long, 4 inches diameter; the length of the wires forming the coils are

(I give M. Ruhmkorff's measurements) stout wire, 30 metres long, 2 millimetres diameter, 200 convolutions; fine wire, 2500 metres long, $\frac{1}{4}$ millimetre diameter, 10,000 convolutions. These measurements will only be taken as approximative, and indeed the exact size is immaterial to the consideration of the experiments which I am about to detail. I will not give my experiments in the order in which I made them, as I should have to describe many fruitless ones, but I will place first that which I consider the most important and fundamental.

1st. On the plate of a good air-pump was placed a silvered copper plate, such as is ordinarily used for Daguerreotypes, the polished silver surface being uppermost. A receiver, with a rod passing through a collar of leathers, was used, and to the lower extremity of this rod was affixed a steel needle, which could thus be brought to any required distance from the silver surface; a vessel containing potassa fusa was suspended in the receiver, and a bladder of hydrogen gas was attached to a stopcock, another orifice enabling me to pass atmospheric air into the receiver in such quantities as might be required*. A vacuum being made, hydrogen gas and air were allowed to enter the receiver in very small quantities, so as to form an attenuated atmosphere of the mixed gas: there was no barometer attached to my air-pump, but from separate experiments I found the most efficient extent of rarefaction for my purpose was that indicated by a barometric height of from half to three-quarters of an inch of mercury; and except where otherwise stated, a similarly attenuated medium was employed for all the following experiments.

Two small cells of the nitric acid battery, each plate exposing 4 square inches of surface, were used to excite the coil machine, and the discharge from the secondary coil was taken between the steel point and the silver plate. The distance between these was generally $=0.1$ of an inch, but this may be considerably varied. When the plate formed the positive terminal, a dark circular stain of oxide rapidly formed on the silver, presenting in succession yellow, orange and blue tints, very similar to the successive tints given by iodizing in the ordinary manner a Daguerreotype plate. Upon the poles being reversed and the plate made negative, this spot was entirely removed, and the plate became perfectly clean, leaving, however, a dark, polished spot occasioned by molecular disintegration, and therefore distinguishable from the remainder of the plate.

The experiment was repeated a great many times, and with varying proportions of gas, and I found that with proportions varying from equal volumes of hydrogen and air to those of one volume of the former to two and a half of the latter, the experi-

* See a figure and description of the apparatus at the end of this paper.

ments succeeded; better, I should say, when there was rather an excess of hydrogen as compared with the equivalent of oxygen in the atmospheric air; about one volume of hydrogen to one and a half of air succeeded well; when excess of air was present, oxidation took place whether the plate was positive or negative, and when excess of hydrogen was present no oxidation took place.

2nd. I experimented with an air vacuum (to borrow an expression of Dr. Faraday), and found that oxidation took place whether the plates were positive or negative, but in different degrees; when the plate was positive, a small circular spot was rapidly formed, quickly deepening in colour, and apparently eating into the plate; when the plate was negative, a large diffuse spot was formed, the oxidation was more slow, and the plate not so rapidly corroded.

3rd. I now operated with a hydrogen vacuum; when the plate was clean no discoloration took place, the plate retained its polish, though after a long continuance of the discharge a molecular change was perceptible, producing a frosted appearance similar to the mercurialized portions of a Daguerreotype.

When the plate had been previously oxidated by the discharge in an air vacuum, the oxidation was rapidly and beautifully cleared off by the discharge in the hydrogen vacuum, and this whether the plate was positive or negative, the effect being, however, better and more rapidly produced in the latter case.

4th. I substituted respectively for the steel needle, wires of copper, silver and platinum, and found the effect produced by all and with nearly equal facility; if there were any difference, the platinum point was the least efficient; this may be due to the peculiar effect of platinum in itself combining the gases, or to its inoxidable character, the oxygen being thrown off from its surface, and not uniting with it as with the more oxidable metals; the flame or luminous appearance which surrounded the wire when the platinum was negative, was larger and more diffuse than with the other metals.

5th. As air, notwithstanding its containing a great excess of nitrogen, gave an effect of oxidation at both electrodes, though different in degree, I increased the proportion of nitrogen by passing into the receiver nitrogen which had been formed by the slow combustion of phosphorus, the phosphorous acid having been well washed away, and potash being always in the receiver; no more air was allowed to be present than the very small quantity contained in the apertures of the stopcock; with this mixture, viz. a maximum of nitrogen and a minimum of oxygen, and rarefied as before, a similar effect was produced to that shown in the mixture of air and hydrogen, the positive plate being oxidated by the discharge, and the spot when made negative being

reduced. The effect of reduction was not so rapid or so readily produced as when hydrogen was used, but was very decided.

6th. With nitrogen, as much deprived of oxygen as I could procure, the colours of oxidation were not exhibited, but a dark spot apparently due to disintegration was produced, which was not removed by the plate being made negative; if, however, the coloured spot was produced by the plate being made positive in an air vacuum, they were removed by the plate being made negative in a nitrogen vacuum, leaving, however, a darker spot than that which was exhibited when they were reduced in hydrogen. Even when produced in an air vacuum, and then a very perfect exhaustion effected, such as would reduce the mercury in the barometer to the height of $\frac{1}{20}$th of an inch, the spot was partially reduced when the plate was made negative.

7th. An oxyhydrogen vacuum was formed, the gases being in the proportion in which they form water; and thanks to the attenuated atmosphere, it was easy to take the discharge in this mixture without producing detonation or any sudden combination of the gases, a possibility pointed out by Grotthus*. With this mixture the effect took place as with the mixture of atmospheric air and hydrogen. I expected it to have been more efficient, but it was rather less so than the mixture of air and hydrogen; whether it be that the presence of nitrogen lessens the tendency to combine of the gases oxygen and hydrogen, and thus enables the electrical polarization and discharge to operate more efficiently, whether the nitrogen has a specific effect in aiding the electro-chemical effect, as I have shown it has in one peculiar case†, or whether any unknown effect of nitrogen is concerned, I do not undertake to pronounce; I can only say that, in several repetitions of the experiment, it appeared to me that the mixture of atmospheric air and hydrogen was more efficient in exhibiting this phænomenon than that of oxygen and hydrogen.

8th. Different proportions of oxygen and hydrogen were employed, and here also I found that within a tolerably wide margin I could vary the proportion of the gases; three volumes of hydrogen to one volume of oxygen I found to be a very efficient mixture.

9th. I now substituted for the silver plate, plates of the following metals:—bismuth, lead, tin, zinc, copper, iron and platinum, the former three metals being burnished, the latter polished.

Bismuth showed the effect nearly, if not quite as well as silver; it was oxidated in an air vacuum, reduced in a hydrogen vacuum, and oxidated or reduced in the mixed gas according as it formed the positive or negative terminal.

* *Annales de Chimie,* vol. lxxxii.   † Phil. Trans. 1843, pp. 110, 111.

Lead oxidated easily, but the spot of oxide could with difficulty be reduced.   Tin, zinc, and copper required the admission of a great quantity of air to produce oxidation; and I could not succeed in reducing the oxide by the electrical discharge, at least so as to restore the polish of the plate; a blackening effect was in some degree produced.   Iron was not oxidated until the receiver was nearly filled with air, and then a small spot of rust was formed which I could not reduce.   With all the metals a slight whitish film like the mercurialized portion of a Daguerreotype was visible beyond the circle marked by the discharge when the plate was rendered positive, which film was removed by negative electrization in a hydrogen vacuum; it seemed to me that this film, as well as others among those I have described, was affected by light, but I did not turn aside to examine this effect.   Platinum showed no effect either of oxidation or reduction.

10th. As it was impossible to operate with an atmosphere of chlorine with the apparatus which I possessed, and wishing to vary the electro-negative element, I iodized a silver plate by the vapour of iodine to a deep blue colour, and then made it negative in an atmosphere of hydrogen; the iodine was beautifully removed in a circle or disc opposite the point which formed the positive terminal.

11th. I now substituted for the coil apparatus a very good electrical machine, the cylinder of which was 16 inches diameter, and the prime conductor of which, when the machine was properly excited, gave a spark of 8 inches long.   With this machine, and in an attenuated atmosphere of one volume hydrogen plus two of atmospheric air, I produced the effects of oxidation and reduction very distinctly, the plate being in turn connected with the conductor and with the ground; but the comparative minuteness of the spot after many turns of the machine, showed the great superiority of the coil machine for producing quantitative effects over the ordinary electrical machine; and I question whether I should have detected the phænomenon with the latter, had I not become previously well acquainted with it by the former apparatus.   Probably an extensive series of the water battery or a steam hydro-electric machine would succeed equally well, or better than the coil machine.

12th. A solution of hyposulphite of soda removed the spots formed by electrization from the silver plate just as it removes the iodine from an iodized plate.

13th. In some of the above experiments I remarked a tendency in the spots produced by the discharge, to show circles or zones of oxidation in different degrees, and in a more marked manner than would be accounted for by the different colours of the thin films of oxide formed.   I determined to examine this effect, and selected, after some experiments, an atmosphere of

one volume oxygen mixed with four volumes of hydrogen, and attenuated by the air-pump as in the previous experiments. The plate was made positive, and the point was placed successively opposite different portions of the silver plate, at distances of $\frac{1}{50}$th, $\frac{2}{50}$ths, $\frac{3}{50}$ths, $\frac{4}{50}$ths, and $\frac{5}{50}$ths of an inch. The results are given, as nearly as I can copy them, in the accompanying Plate V. figs. 1 to 5.

The colour of the central spot was a yellow-green in the centre, surrounded by a blue-green, then a clear ring of polished silver, then an outer ring crimson, with a slightly orange tint on the inner side, and deep purple on the outer; the exterior portion of the spot was, as far as my eye could judge, of a colour complementary to the interior of the external ring, and the central portion of the spot of a colour complementary to the exterior portion of the ring. The colours varied with the time, density of gas and other conditions, but generally showed this complementary tendency. Symptoms of a faint polished ring were visible beyond the outer ring, and could be rendered more distinct by breathing on the plate. As the distance between the point and the plate was increased, the colours became fainter, and the rings more diffuse, and beyond the distance I have given nearly lost their defined character; but the first three distances, or those of $\frac{1}{50}$th, $\frac{2}{50}$ths, and $\frac{3}{50}$ths of an inch, gave very beautifully defined rings. The luminous appearance on the needle in these experiments extended from three-fourths of an inch to an inch from the point. Frequently a small polished speck was visible, exactly opposite the point of the needle. See fig. 6. When the plate was made negative, the other conditions being the same, a polished space appeared opposite the point of the needle, surrounded by a dusky and ill-defined areola; its colour, when regarded from a point opposite the incident light, was brown tinged with purple; and when in the same direction as the light, a greenish white, similar to the tint seen on mildew or on some of the lichens: these spots were very different from the positive spots, and in some degree the converse of them; but they were not nearly so well defined or capable of being produced with the same uniformity. I have endeavoured to represent one of them at fig. 7.

14th. In order to ascertain whether the polished ring intervening between the oxidated central spot and oxidated external ring were a mere negation of effect or an antithetic polar effect, such as would occasion reduction, I formed in an air vacuum two large spots on a silver plate, with one the plate being made negative, and with the other positive, oxidating them until they began to pass from deep orange to purple. I then perfectly exhausted the receiver, swept it with the gas employed in the last experiment, and then took the discharge in a vacuum of that

gas, viz. one volume oxygen + four hydrogen; the plate being positive and the needle $\frac{2}{50}$ths of an inch over the centre of each spot in turn, a ring of clear polish was formed rapidly in both the dark discs, just at the distance where the ring of polish appeared in the last experiment. I then exposed a clean portion of the plate to the needle without any other change, and on allowing the discharges to pass, formed the rings just as in the last experiment.

15th. I examined some of the spots with an achromatic microscope, magnifying 200 diameters; I could not, however, discover any feature which the naked eye did not show, or any peculiar molecular state; the polishing scratches on the plate were highly magnified, but the electrized spots only showed more dimly the colours or the lights and shadows which they exhibited to the naked eye.

16th. I took the discharge on a silver plate in vacua of the following gases respectively:—Oxygen, protoxide of nitrogen, deutoxide of nitrogen, carbonic acid, carbonic oxide and olefiant gas.

The first four gases presented nothing remarkable; the plate was oxidated whether positive or negative, as in a vacuum of atmospheric air. In the protoxide of nitrogen the colour of the discharge was a beautiful crimson on both terminals.

In deutoxide of nitrogen a greater tendency to reduction was shown when the plate was negative than in the other three gases, and there was also a tendency to the formation of rings. In carbonic oxide the plate was oxidated when positive, and the oxide reduced when negative, just as with a vacuum of air and hydrogen, but rather more slowly; with a mixture of five volumes of carbonic oxide and one volume of oxygen, the rings were formed very distinctly, particularly if the plate was made negative first, and then positive. The luminous spot on the plate, when positive in this gas, was coloured green.

When the plate was negative in olefiant gas it darkened, showing the rings of colour produced by thin plates, and very distinct from the other rings of which I have spoken. After a short time a pulverulent deposit was formed on the plate, giving brilliant sparks or stars of light which were not shown by any other gas.

This deposit was too minute for analysis; but I have no doubt, from the gas used and the appearances presented, it was carbon.

I have given in the above experiments the conditions under which they succeeded best; but upon repetition, although the exact volumes of gases and other conditions were carefully attended to, they sometimes required a slight alteration to succeed, variations taking place from causes which I could not detect; thus it was sometimes necessary to add a little more

hydrogen, sometimes a little more oxygen or air, to alter slightly the state of attenuation in the gas, &c.

The necessarily varying condition of the battery, and the state of the contact breaker, slight impurities in the gases or on the surface of the plates would be quite sufficient to account for these irregularities. I mention them for the guidance of any one who may wish to repeât the experiments; a very little practice will enable any electrician to have the results at his command. When there is too great a proportion of air or oxygen, oxidation takes place at both poles; when too much hydrogen, reduction takes place at both; and to effect oxidation or reduction by reversing the direction of the discharge, an intermediate condition is requisite; so if the gas be not sufficiently attenuated, the oxidation is too rapid, and the plate too much corroded, to bring out the effects clearly; if too much attenuated, too long a time is required, and the effect is feeble and indistinct.

I have above selected all the experiments which I consider material in this, I believe, new class of phænomena. The spots produced by electrical discharges, both on conducting bodies and on electrics, have been before noticed and experimented on; one class by Priestley[*], and another class by Karsten[†] and others; but as far as I am aware, no distinct electro-chemical action in dry gases, depending upon the antithetic state of the terminals and presenting a definite relation of the chemical to the electrical actions in gaseous media, has been pointed out. I now proceed to consider the relation which these results bear to other electrical phænomena.

As may be gathered from my opening remarks, the experiments above detailed appear to me to furnish a previously deficient link in the chain of analogy connecting dielectric induction with electrolysis. The only satisfactory rationale which I can present to my own mind of these phænomena is the following. The discharges being interrupted (as is evident from the nature of the apparatus, and may be easily proved by agitating a mirror near them and regarding their reflected images in the moving mirror), the gaseous medium is polarized anterior to each discharge, and polarized not merely physically, as is generally admitted, but chemically, the oxygen or anion being determined to the positive terminal or anode, and the hydrogen or cation being determined to the negative terminal or cathode; at the instant preceding discharge there would then be a molecule or superficial layer of oxygen or of electro-negative molecules in contact with the anode, and a similar layer of hydrogen or of electro-positive molecules in contact with the cathode, in other words,

* History of Electricity, 2nd edition, p. 624.
† *Archives de l'Electricité*, vol. ii. p. 647; vol. iii. p. 310.

the electrodes in gas would be polarized as the electrodes in liquid are. The discharge now takes place, by which the superficial termini of metal or of oxide, as the case may be, are highly ignited or brought into a state of chemical exaltation at which their affinities can act; the anode thus becomes oxidated, and the cathode, if an oxide, reduced. I have elsewhere* shown strong reasons for assuming that the electric or voltaic discharge, the moment polarity is subverted, may be regarded as an intensely heated state of the electrodes, and of the intermedium across which it passes; and my present explanation is perfectly consistent with, and derivable from, my previous views of the disruptive discharge.

Two other theories might be proposed to account for the phænomena I am considering; the one, that the disruptive discharge itself is analogous to the electrolytic, and that the oxygen and hydrogen are reciprocally transferred by the discharge itself; this would not, I think, be consistent with the generally known facts connected with the discharge, and is entirely ineffectual in explaining the Experiments 2 and 3, where either the positive or negative terminal can be made either to oxidate or reduce, according to the nature of the chemical medium present, while these experiments are entirely in accordance with, and the results of them flow as a necessary consequence of, the view first advanced. The other theory which may be advanced is, that by dielectric induction the gases may be bodily separated, a layer, not molecular, but corporeal or voluminous, if I may be allowed these expressions, of oxygen being developed on the side next the anode, and one of hydrogen next the cathode, the gas intervening between the terminals being thus divided, as it were, into two halves: this would certainly be a most curious phænomenon, but I believe it to be so inconsistent with the vast mass of accumulated facts in electrical science, and likely to have produced in cosmical phænomena so many results which, if existing, must long ere this have been detected, that I will not do more than advert to it.

I have adopted the views which I have first stated as being the least removed from ordinary theories or modes of regarding electrical phænomena, and because in the present instance I can present the phænomena in no other way which is in the least degree satisfactory to my own mind, while this view to me well accounts for them. Assuming then for the present this view, we get a close approximation, I may say an identity of the state of polarization in gaseous non-conducting dielectrics, and in electrolytes anterior respectively to discharge or to electrolysis.

* Philosophical Transactions, 1847, pp. 10, 16, 21. Correlation of Physical Forces, p. 50, 2nd edition.

Faraday observes, Experimental Researches, 1164, "In an electrolyte induction is the first state, and decomposition the second." My present experiments show, I believe, that in induction across gaseous dielectrics there is a commencement, so to speak, of decomposition, a polar arrangement not merely of the molecules, irrespective of their chemical characters, but a chemical alternation of their forces, the electro-negative element being determined or directed, though *not travelling* in one direction, and the electro-positive in the opposite direction.

This arrangement is only evidenced at present, as it is in electrolysis, by the action at the polar extremities or termini of the dielectric; possibly future researches may show, by the action of polarized light, by magnetism or some other means of analysis, that the polarity extends, as we theoretically believe it does, through the whole intervening matter.

In the Experiment No. 5 with oxygen and excess of nitrogen, reduction takes place by the effect of negative electricity and heat, at least there seems every reason from analogy to believe that the effect of the nitrogen is only negative, protecting the plate from oxygen, or at furthest catalytic, aiding the reduction as sulphuric acid aids the electrolysis of water. Upon the state of association of the gases in what is generally called mixture, I venture an opinion with the greatest diffidence. I have always inclined to the opinion that the difference between physical admixture, as it is termed, of gases and chemical union, is one of degree, and the views of Dalton ever presented to my mind grave difficulties*. My present results seem to me in favour of the chemical view, as otherwise we can scarcely imagine electricity as effecting in the instances given a merely physical separation; it may indeed be said that there is composition and decomposition produced by the same discharge; but this is very difficult to conceive, and can hardly apply to the cases of oxygen with nitrogen and of carbonic oxide.

In the experiments I have detailed, the flame or visible effect of the electric discharge coincided with the chemical effect; when the plate was positive, a small globule of flame of a purple colour was visible on the part of the plate attacked, and a bluish flame extended over an inch or more of the needle. When the plate was negative, a wider and less-defined disc of blue flame extended over the part of the plate opposed to the positive point, like a splash of liquid thrown upon it, and a pencil of light appeared on the point. Sometimes, but not always, this flame avoided the oxidated portion, probably from its inferior conducting power; and when this was the case, reduction took place in a much slighter degree, or not at all; sometimes, and I observed

* Philosophical Transactions, 1843, p. 112.

this particularly with bismuth, the flame attached itself to the oxidated portion, and then reduction immediately followed. Here, as in all the electrical phænomena that I can call to mind, we get the visible effects of electricity associated with physical changes in the matter acting, changes of state in the terminals, polarization of the intervening medium, or both[*]. These experiments furnish additional arguments for the view which I have long advocated, which regards electricity as force or motion, and not as matter or a specific fluid[†].

The chemical polarity of gases shown, as I believe, in this paper, associates itself with an experiment which I made known in a lecture at the London Institution in the year 1843[‡], and which was subsequently verified by Mr. Gassiot[§] with more perfect apparatus than I possessed, viz. that when discs of zinc and copper are closely approximated, but not brought in contact, and then suddenly separated, effects of electrical tension are exhibited, the one disc making the electroscope diverge with positive, and the other with negative electricity, showing that the effects ascribed by Volta to contact can be produced without contact, and by mere approximation, the intermediate dielectric being polarized, or a radiation analogous, if not identical, with that which produces the images of Moser taking place from plate to plate.

The present experiments also associate themselves with the gas battery, where, though an electrolyte is used as the means of making the action continuous, or producing what is called current electricity, the initiating effect is gaseous polarity, the films of gas in contact with the respective plates of platinum having antithetic chemical and electrical states.

The results detailed in Experiment 13, appear to open a new field of research. Priestley observed concentric circles produced by the electrical discharge from a powerful Leyden battery, which he describes as consisting of minute cavities and globules of fused metal[||]. In my experiments there is an alternation of oxidation and reduction, a medium capable of producing both being present; the lateral effect and complementary colours have to my mind something closely resembling the phænomena of interference in light, although from the polar character of the

* Gases at present believed to be elementary, probably undergo a *quasi* chemical polarization by electricity; thus portions of oxygen are changed to ozone, &c. See a recent paper by MM. Fremy and E. Becquerel, *Comptes Rendus*, Paris, March 15. [Phil. Mag. July 1852, p. 543.]—Note added to the Proof, W. R. G.

† Printed Lecture at the London Institution, 1842, p. 28. Correlation of Physical Forces, p. 48.

‡ Literary Gazette, 1843, p. 39.   § Phil. Mag., October 1844.

|| History of Electricity, 2nd edition, p. 624.

force, it is difficult to imagine any precisely analogous condition of electricity. The discharge taking place from different parts of the needle and extending from its point to a considerable distance over its surface, would give different lengths for the lines of polarization and discharge to the different parts of the disc on the silver plate affected by the discharge; and assuming electricity to be propagated by undulations, there would be interference; but instead of alternations of light and darkness we get alternations of positive and negative electricity. The ring of polished metal between the central spot and the exterior ring, quite distinguishes these rings from the ordinary colours of thin plates, *i. e.* colours, the annular succession of which depends only on the different thicknesses of the film; here doubtless the colours of the oxidated portions are colours of thin plates. Experiment 14 shows clearly that the action by which the polished ring is formed is a polar action of the discharge, and not a mere absence of action.

When the plate is negative, the effect is, as I have observed, less marked and more uncertain; but in this case it should be recollected that the visible discharge issues from the point, and does not extend, or extends to a very small degree, over the surface of the needle.

If the phænomena were such that the central portion were always clear, while around it was one, and one only circle of oxide, it might be accounted for by the hypothesis, that the lines of polarization and discharge between a point and flat surface assume the form of a hollow cone; but a cone of negative bounded by cones of positive action, still gives the idea of some lateral fits or phases of undulation.

The high rarefaction of the medium by the discharge, and its intermitting character, might occasion pulsations by the inrushing of the surrounding gas, and thus vacua in circles might be formed at the places where the action of oxidation is rendered null; but this view is, I think, inadmissible; it does not account for the effects obtaining only in certain mixtures, it does not account for the reducing action, the plate being positive, and presents other difficulties. The point involved in Experiments 13 and 14, though not perhaps the least valuable one given in this paper, presents apparently a wide field of inquiry; I therefore will not further dilate on it at present, and hope to make it the subject of future investigation.

December 27, 1851.

Postscript, April 24th.

I may, I trust, be permitted to add to this paper one or two experiments on the subject last discussed. Assuming that the

alternations of oxidation and reduction were produced by interference in consequence of the discharge proceeding from successive points of the terminal or terminals, a difference of effect might be anticipated if the electricity passed from a point only, and not from a line as was the case in Experiment 13. I therefore sealed a platinum wire $\frac{1}{60}$th of an inch in diameter into a piece of glass tubing, and then ground the extremity to a flat surface, so that the section only of the wire was exposed; this wire was placed opposite, and at 0·07 of an inch distance from the polished silver plate, in a mixture of one volume of oxygen with five volumes of hydrogen attenuated until the barometer stood at half an inch; discharges from the secondary coil were then passed, the plate being positive, and a round dark spot of oxide formed represented at fig. 8; the platinum sealed in glass was then removed and the steel needle substituted for it, all else, viz. plate, gas, barometer height, &c. being the same : the system of rings represented at fig. 9 was now produced.

Another experiment was made, directed to the same point : a wire of copper 0·04 inch diameter, and a thread of glass of the same diameter were attached by sealing-wax at their extremities in a horizontal position 0·025 of an inch from different parts of a silver plate, being insulated from the silver by the wax interposed at the extremities. The gaseous mixture and barometric height being the same as in the last experiment, and the silver plate made positive, when the platinum wire sealed in glass was brought near the plate, and the discharges passed, a spot similar to fig. 8 was formed; but when the coated point of platinum was brought over the copper wire at 0·02 inch distance, a figure consisting of two separated semicircles was formed, having spots in the bisection of the chords, as shown at fig. 10, the portion between the spots and the semicircular line of oxide being of polished silver. With the glass thread the effect was the same, but produced with greater difficulty and not so well defined.

In many repetitions of these experiments which I have made, I have invariably produced the alternately polished and oxidated rings from the bare wire, and have not procured them from the coated wire, except to a very slight degree, and under certain circumstances, which, as far as I could trace, were as follows :—

1st. When the extremity of the wire was very near the plate, so that it had a sensible magnitude with reference to the intervening space, a slight formation of minute rings could be detected at the commencement of the experiment.

2nd. When the experiment was long continued, or when the coated platinum wire had been used for previous experiments, a set of rings, not consisting of an alternation of oxidated and

polished rings, but of annuli of different degrees of oxidation, were formed.

When the experiment is continued for some time, a dark deposit is formed on the glass around the extremity of the platinum wire, giving an extended conducting surface; and this may be the reason why such rings are formed, though these rings, in all the cases which I have observed, differ broadly from the rings formed by the bare needle or wire, not having the interposed spaces of perfectly bright silver; and in all the cases the difference of effect produced by the coated and the bare wire is very marked; in by far the greater number of experiments, when proper precautions are taken, not the slightest formation of rings takes place with the coated wire; with the bare wire, in the gaseous mixture last mentioned, I have always seen them formed.

Thus there are three systems of rings which may be formed by the discharge. First, rings such as those seen in the ordinary cases of thin plates; these I have only observed with olefiant gas, though probably there are many other conditions in which they may be produced. Secondly, rings formed by the superposition of layers of oxides, possibly arising from the fact that at certain definite periods portions of the plate become by oxidation inferior conductors, and other portions are attacked, and being at a different distance undergo a different molecular change by oxidation. Thirdly, and to me far the most interesting set of phænomena are presented by the rings alternately bright and oxidated, showing effects of oxidation and reduction by the same current on the same plate, and which only take place in certain gaseous mixtures, of which, up to this time, one volume oxygen + five volumes hydrogen is the most efficient which I have obtained.

I cannot at present see any better mode of explaining these phænomena than by regarding them as analogous to the phænomena of interference in light; though doubtless if this be a right view, the very different modes of action of light and electricity would present very numerous phænomenal distinctions. Alternations of opposite polar electrical actions in the discharges passing in the same direction are, I think, very clearly shown in these experiments, and this appears to me a result worthy of attention.

Though acquainted with Nobili's beautiful experiments on the formation of coloured rings by deposition in electrolyzed liquids, yet as I was working on gases it did not occur to me to refer to his memoirs*; I have done so since making the experiments given in this postscript, and find that with regard to the rings so formed by electrolysis, he suggests interference as a possible explanation.

The dark space in the discharge to which Faraday has called

* *Ann. de Ch. et de Phys.* vol. xxxiv.

Phil. Mag. S. 4. No. 28. *Suppl.* Vol. 4.   2 L

attention, may possibly be connected with these phænomena. I have observed, that in a well-exhausted receiver containing a small piece of phosphorus, the discharge is throughout its course striated by transverse non-luminous bands, presenting a very beautiful effect, and a yellow deposit, which, as far as I have yet examined it seems to be allotropic phosphorus, is deposited on the plate of the air-pump and on the neighbouring substances; to show this effect well the needle should be positive and the plate negative, and the distance between them about an inch.

I could dilate much further on these experiments, but have already trespassed perhaps too far for a postscript. Variations in the form of the terminals, in the nature of the gas, vapour, or gaseous mixture, in the density of the gas, in the intensity and quantity of the discharge, in the nature of the plate, &c., will occur to those who may feel inclined to repeat these experiments, and if I am not over-sanguine, promise results of much interest.

Additional Note on the dark discharge, July 9, 1852.

I find the transverse dark bands can be produced in other gases when very much attenuated, probably in all; and I rather think the reason why they are more easily seen in the phosphorus vapour is, that all the oxygen having been consumed a better vacuum is formed.

In addition to these bands, and under circumstances where they are barely visible, there is always seen a well-defined dark space intervening between the glow surrounding the negative, and the stream of light proceeding from the positive terminal; it appears independently of the length of the discharge, though a space of an inch is a convenient distance for exhibiting the effect well.

This dark discharge is elaborately described by Faraday as produced by the ordinary electrical machine, Experimental Researches, § 1544 *et seq.*

Having in my mind the analogy of interference, it seemed to me that this dark space might be due to the crossing of the lines of discharge from the successive points of the needle, the knob, or plate from which the negative discharge issues.

As the positive discharge appears to issue from a point, and not to surround the wire, as does the negative, the position of the dark space in close approximation to the negative terminal was in favour of this view; if correct, it should follow that if the terminals were coated points instead of wires, knobs or plates, this dark space would not be observed, or its position would be changed. Experiment verified this expectation: when platinum wires sealed in glass were employed and a good vacuum formed, the line of luminous discharge was continuous when the platinum points were brought to a distance of half an inch.

When these terminals are so far separated as not to give a continuous line of discharge, a pencil appears on each terminal, which gradually becomes fainter and fainter towards the middle of the intervening space; and if the distance be great, the discharge ceases to be luminous towards the middle of the intervening space, from excessive diffusion; but this will be seen to be a very different effect from the abrupt and well-defined dark space which appears in close approximation to the negative terminal when the coated wires are not employed.

When the positive terminal is coated and the negative one bare, the dark space appears on the point of the bare wire, the wire itself being surrounded by a lambent flame; but with the converse arrangement there is no such dark space. All this is much in favour of interference taking place, the coincidence of positive and negative phases of the discharge producing at certain points mutual neutralization.

Description of Plate.
Plate V.

Figures 1 to 10 show the spots and rings in the order referred to : it should be observed that printed figures give but a very imperfect notion of the actual effects.

Fig. 11 is the coil apparatus, the contact breaker being in front.

Fig. 12. The air-pump, of a construction which I proposed many years ago, and have found most useful for electrical or chemical experiments on gases.

    P. An inperforate piston, with a conical end, which, when pressed down, fits accurately the end of the tube, the apex touching the valve V, which opens outwards.

    A. Aperture for the air to rush from the receiver when the piston has been drawn beyond it.

    B. Bladder containing the gas to be experimented on.

    The piston-rod works air-tight in a collar of leathers, and the operation of the pump will be easily understood without further description.

    If it be required to examine the gas after experiment, a bladder, or tube leading to a pneumatic trough, can be attached at the extremity over the valve V.

LXXX. *Demonstration of a Theorem relating to the Products of Sums of Squares.* By Arthur Cayley[*].

MR. KIRKMAN, in his paper "On Pluquaternions and Homoid Products of Sums of n Squares" (Phil. Mag. S. 3. vol. xxxiii. p. 447), quotes from a note of mine the following passage :—"The complete test of the possibility of the product of 2^n squares by 2^n squares reducing itself to a sum of 2^n squares is the following : forming the complete systems of triplets for $(2^n - 1)$ things, if *eab, ecd, fac, fdb* be any four of them, we must have, paying attention to the signs alone,

$$(\pm eab)(\pm ecd) = (\pm fac)(\pm fdb) ;$$

i. e. if the first two are of the same sign, the last two must be so also, and *vice versâ*; I believe that, for a system of seven, two conditions of this kind being satisfied would imply the satisfaction of all the others : it remains to be shown that the complete system of conditions cannot be satisfied for fifteen things." I propose to explain the meaning of the theorem, and to establish the truth of it, without in any way assuming the existence of imaginary units.

The identity to be established is

$$(w^2 + a^2 + b^2 + \ldots)(w_i^2 + a_i^2 - b_i^2 \ldots)$$
$$= w_{ii}^2 + a_{ii}^2 + b_{ii}^2 + \ldots$$

where the 2^n quantities $w, a, b, c \ldots$ and the 2^n quantities $w_i, a_i, b_i, c_i \ldots$ are given quantities in terms of which the 2^n quantities $w_{ii}, a_{ii}, b_{ii}, c_{ii} \ldots$ have to be determined.

Without attaching any meaning whatever to the symbols $a_o, b_o, c_o \ldots$ I write down the expressions

$$w + aa_o + bb_o + cc_o \ldots, \quad w_i + a_i a_o + b_i b_o + c_i c_o \ldots,$$

and I multiply as if $a_o, b_o, c_o \ldots$ really existed, taking care to multiply without making any transposition in the order *inter se* of two symbols a_o, b_o combined in the way of multiplication. This gives a quasi-product

$$ww_i + (aw_i + a_i w)a_o + (bw_i + b_i w)b_o + \ldots$$
$$+ aa_i a_o^2 + bb_i b_o^2 + \ldots$$
$$+ ab_i a_o b_o + a_i b\, b_o a_o + \ldots$$

Suppose, now, that a quasi-equation, such as

$$a_o b_o c_o = +$$

means that in the expression of the quasi-product

$$b_o c_o, \quad c_o a_o, \quad a_o b_o, \quad c_o b_c, \quad a_o c_o, \quad b_o a_o$$

are to be replaced by

$$a_c, \quad b_o, \quad c_o, \quad -a_c, \quad -b_o, \quad -c_o;$$

and that a quasi-equation, such as $a_o b_o c_o = -$, means that in the expression of the quasi-product

$$b_o c_o, \quad c_o a_o, \quad a_o b_o, \quad c_o b_o, \quad a_o c_o, \quad b_o a_o$$

are to be replaced by

$$-a_o, \quad -b_o, \quad -c_o, \quad a_o, \quad b_o, \quad c_o.$$

It is in the first place clear that the quasi-equation $a_o b_o c_o = +$ may be written in any one of the six forms

$$a_o b_o c_o = +, \quad b_o c_o a_o = +, \quad c_o a_o b_o = +$$
$$a_o c_o b_o = -, \quad c_o b_o a_o = -, \quad b_o a_o c_o = -;$$

and so for the quasi-equation $a_o b_o c_o = -$. This being premised, if we form a system of quasi-equations, such as

$$a_o b_o c_o = \pm, \quad a_o d_o e_o = \pm \,\&\text{c.},$$

where the system of triplets contains each duad once, and once only, and the arbitrary signs are chosen at pleasure; if, moreover, in the expression of the quasi-product we replace $a_o^2, b_o^2, \ldots$ each by -1, it is clear that the quasi-product will assume the form

$$w_{ii} + a_{ii} a_o + b_{ii} b_o + c_{ii} c_o + \ldots,$$

$w_{\prime\prime}, a_{\prime\prime}, b_{\prime\prime}, c_{\prime\prime} \ldots$ being determinate functions of $w, a, b, c, \ldots$; $w_{\prime}, a_{\prime}, b_{\prime}, c_{\prime} \ldots$ homogeneous of the first order in the quantities of each set; the value of $w_{\prime\prime}$ being obviously in every case

$$w_{\prime\prime} = ww_{\prime} - aa_{\prime} - bb_{\prime} - cc_{\prime} \ldots$$

and $a_{\prime\prime}, b_{\prime\prime}, c_{\prime\prime} \ldots$ containing in every case the terms $aw_{\prime} + a_{\prime}w$, $bw_{\prime} + b_{\prime}w$, $cw_{\prime} + c_{\prime}w, \ldots$ but the form of the remaining terms depending as well on the triplets entering into the system of quasi-equations as on the values given to the signs $\pm$; *the quasi-equations serving, in fact, to prescribe a rule for the formation of certain functions* $w_{\prime\prime}, a_{\prime\prime}, b_{\prime\prime}, c_{\prime\prime} \ldots$, the properties of which functions may afterwards be investigated.

Suppose, now, that the system of quasi-equations is such that

$$e_\circ a_\circ b_\circ, \quad e_\circ c_\circ d_\circ$$

being any two of its triplets, with a common symbol e_c, there occur also in the system the triplets

$$f_\circ a_\circ c_\circ, \quad f_\circ d_\circ b_\circ; \quad g_\circ a_\circ d_\circ, \quad g_\circ b_\circ c_\circ;$$

and suppose that the corresponding portion of the system is

$$e_\circ a_\circ b_\circ = \epsilon, \quad e_\circ c_\circ d_\circ = \epsilon'$$
$$f_\circ a_\circ c_\circ = \zeta, \quad f_\circ d_\circ b_\circ = \zeta'$$
$$g_\circ a_\circ d_\circ = \iota, \quad g_\circ b_\circ c_\circ = \iota',$$

where $\epsilon, \zeta, \iota, \epsilon', \zeta', \iota'$ each of them denote one of the signs $+$ or $-$; then $e_{\prime\prime}, f_{\prime\prime}, g_{\prime\prime}$ will contain respectively the terms

$$\epsilon(ab_{\prime} - a_{\prime}b) + \epsilon'(cd_{\prime} - c_{\prime}d)$$
$$\zeta(ac_{\prime} - a_{\prime}c) + \zeta'(db_{\prime} - d_{\prime}b)$$
$$\iota(ad_{\prime} - a_{\prime}d) + \iota'(bc_{\prime} - b_{\prime}c);$$

and $e_{\prime\prime}^2 + f_{\prime\prime}^2 + g_{\prime\prime}^2$ contains the terms

$$(a^2 + b^2 + c^2 + d^2)(a_{\prime}^2 + b_{\prime}^2 + c_{\prime}^2 + d_{\prime}^2.) - a^2a_{\prime}^2 - b^2b_{\prime}^2 - c^2c_{\prime}^2 - d^2d_{\prime}^2$$
$$+ 2\big[\epsilon\epsilon'(ab_{\prime} - a_{\prime}b)(cd_{\prime} - c_{\prime}d)$$
$$+ \zeta\zeta'(ac_{\prime} - a_{\prime}c)(db_{\prime} - d_{\prime}b)$$
$$+ \iota\iota'(ad_{\prime} - a_{\prime}d)(bc_{\prime} - b_{\prime}c)\big].$$

And by taking account of the terms $ew_{\prime} + e_{\prime}w, fw_{\prime} + f_{\prime}w, gw_{\prime} + g_{\prime}w$ in $e_{\prime\prime}, f_{\prime\prime}, g_{\prime\prime}$ respectively, we should have had besides in $e_{\prime\prime}^2 + f_{\prime\prime}^2 + g_{\prime\prime}^2$ the terms

$$(e^2 + f^2 + g^2.)w_{\prime}^2(e_{\prime}^2 + f_{\prime}^2 + g_{\prime}^2)w^2.$$
$$+ 2(ee_{\prime} + ff_{\prime} + gg_{\prime})ww_{\prime}.$$

Also $w_{\prime\prime}^2$ contains the terms

$$w^2w_{\prime}^2 + a^2a_{\prime}^2 + b^2b_{\prime}^2 + c^2c_{\prime}^2 + d^2d_{\prime}^2.$$
$$- 2(ee_{\prime} + ff_{\prime} + gg_{\prime})ww_{\prime};$$

whence it is easy to see that

$$w_{ii}^2 + a_{ii}^2 + b_{ii}^2 + c_{ii}^2 + \ldots = (w^2 + a^2 + b^2 + c^2 \ldots)$$
$$(w_i^2 + a_i^2 + b_i^2 + c_i^2 \ldots)$$
$$+ 2\Sigma[\epsilon\epsilon'(ab_i - a_i b)(cd_i - c_i d)$$
$$+ \zeta\zeta'(ac_i - a_i c)(db_i - d_i b)$$
$$\iota\iota'(ad_i - a_i d)(bc_i - b_i c)].$$

where the summation extends to all the quadruplets formed each by the combination of two duads such as *ab* and *cd*, or *ac* and *db*, or *ad* and *bc*, *i. e.* two duads, which, combined with the same common letter (in the instances just mentioned *e*, or *f*, or *g*), enter as triplets into the system of quasi-equations—so that if $\nu = 2^n - 1$, the number of quadruplets is

$$\frac{1}{2}\left(\frac{\nu-1}{2} \; \frac{\nu-3}{2}\right)\nu\frac{1}{3} = \frac{\nu(\nu-1)(\nu-3)}{24}.$$

And the terms under the sign Σ will vanish identically if only

$$\epsilon\epsilon' = \zeta\zeta' = \iota\iota',$$

but the relation $\epsilon\epsilon' = \iota\iota'$ is of the same form as the equation $\epsilon\epsilon' = \zeta\zeta'$; hence if all the relations

$$\epsilon\epsilon' = \zeta\zeta'$$

are satisfied, the terms under the sign Σ vanish, and we have

$$(w_{ii}^2 + a_{ii}^2 + b_{ii}^2 + c_{ii}^2 \ldots) = (w^2 + a^2 + b^2 + c^3 \ldots)(w_i^2 + a_i^2 + b_i^2 + c_i^2 \ldots)$$

which is thus shown to be true, upon the suppositions—

1. That the system of quasi-equations is such that

$$e_o a_o b_o, \quad e_o c_o d_o$$

being any two of its triplets with a common symbol e_o, there occur also in the system the triplets

$$f_o a_o c_o, \quad f_o d_o b_o$$
$$g_o a_o d_o, \quad g_o b_o c_o.$$

2. That for any two pairs of triplets, such as

$$e_o a_o b_o, \; e_o c_o d_o \text{ and } f_o a_o c_o, \; f_o d_o b_o,$$

the product of the signs of the triplets of the first pair is equal to the product of the signs of the triplets of the second pair.

In the case of fifteen things $a, b, c \ldots$ the triplets may, as appears from Mr. Kirkman's paper, be chosen so as to satisfy the first condition; but the second condition involves, as Mr. Kirkman has shown, a contradiction; and therefore the product

of two sums, each of them of sixteen squares, is *not* a sum of sixteen squares. It is proper to remark, that this demonstration, although I think rendered clearer by the introduction of the idea of the system of triplets furnishing the rule for the formation of the expressions $w_{ll}, a_{ll}, b_{ll}, c_{ll}$, &c., is not in principle different from that contained in Prof. Young's paper on an Extension of a Theorem of Euler's, &c., Irish Transactions, vol. xxi.

2 Stone Buildings, Oct. 8, 1852.

LXXXI. *On the Theory of Compound Colours.*
By H. HELMHOLTZ[*].

L UMINOUS rays of different wave-length and colour distinguish themselves in their physiological action from tones of different times of vibration, by the circumstance that every two of the former, acting simultaneously upon the same nervous fibres, give rise to a simple sensation in which the most practised organ cannot detect the single composing elements, while two tones, though exciting by their united action the peculiar sensation of harmony or discord, are nevertheless always capable of being distinguished singly by the ear. The union of the impressions of two different colours to a single one is evidently a physiological phænomenon, which depends solely upon the peculiar reaction of the visual nerves. In the pure domain of physics such a union never takes place objectively. Rays of different colours proceed side by side without any mutual action, and though to the eye they may appear united, they can always be separated from each other by physical means.

The investigation of this compound action has led to the theory of primitive colours, from the combination of which all others are, or can be, obtained. This theory, however, has been based from the beginning upon a single mode of experiment; namely, that in which colouring substances are mixed together, the results being assumed to be the same as would follow from the union of the coloured lights themselves,—an assumption the untruth of which I purpose in the following pages to prove.

Pliny mentions the fact that the oldest Greek painters were able to represent all things by means of four pigments, while in his time a much greater number was made use of, but without the ability to produce an equal effect. Leonardo da Vinci, equally celebrated as an artist as for his scientific treatment of painting, was not aware of the theory of the three so-called primitive colours; besides black and white, which however are

strictly speaking not colours, he mentions four, namely yellow, green, blue, and red.   The primitive colours red, yellow, and blue, afterwards generally recognized, have been made the basis of an experiment of Waller described in the Philosophical Transactions for 1686, hence before the time of Newton's investigations on the decomposition of white light by the prism, and when no other method of compounding colours save that of mixing the colouring matters was known.   In later attempts to classify the natural colours according to their composition by the three primitive ones above mentioned, Castell, the astronomer Mayer, Lambert, Hay, and Forbes*, have all taken as basis of their endeavours the mixing of the colouring matters.   As representants of the primitive colours from which all others might be formed, Mayer made use of cinnabar, kings-yellow, and mountain-blue ; Lambert, carmine, gamboge, and Prussian-blue, which give purer mixtures ; and Hay, whose skill in the choice and use of colours for this purpose is praised by Forbes, carmine, chrome-yellow, and French ultramarine.

Some physicists attempted to demonstrate the objective existence of the three primitive colours.   Mayer was the first to give utterance to the view that the three primitive colours might correspond to three different kinds of light, red, yellow, and blue, each of which furnished rays of all degrees of refrangibility. According to this, at every point of the spectrum red, yellow and blue rays are mixed together, which however do not differ in refrangibility, and therefore cannot be separated by the prism.

At the red end of the spectrum the red light was supposed to be predominant, at the blue end the blue, in the middle the yellow.   The same view was afterwards expressed by Brewster ; and this celebrated physicist imagined that he was able, by absorption in transparent coloured media, actually to separate the different kinds of light in all parts of the spectrum.

After his discovery of the composition of white light, Newton assumed the existence of seven principal colours in the spectrum : red, orange, yellow, green, blue, indigo, violet.   He chose this number probably because of the analogy which he sought between the colours and the musical intervals of the scale, and this also suggested the divisions of his seven-coloured disk.   This accounts for the distinction which he has drawn between blue and indigo-blue.   That this distinction has been made in the blue is, in all likelihood, to be referred to the fact that in most prisms the blue portion is comparatively expanded, and the breadth of the bands

* T. Castell, *Farbenclavier.*   Mayer in *Göttinger gel. Anzeigen* 1758, p. 147.   J. H. Lambert, *Beschreibung einer Farbenpyramide*: Berlin, 1772. D. R. Hay, *Nomenclature of Colours.*   J. D. Forbes in Phil. Mag. S. 3 . vol. xxxiv. p. 161.

was compared by Newton to the intervals of the scale. Besides this he must have been content with very incomplete apparatus, and could therefore make but few observations on the artificial union of two or more prismatic colours; the results of these seemed, on the whole, to correspond with those obtained from the mixture of coloured substances. Besides these experiments he made others on the mixture of coloured powders.

Newton always obtained his spectra from sunlight, and did not apply the methods necessary for the complete separation of the differently coloured rays; hence it is that he did not observe the lines of Fraunhofer in the spectrum. Wollaston* was the first to obtain a spectrum so pure as to permit of a few of these lines being seen in it. Through a very good flint-glass prism he looked with his naked eye at a fine slit, through which diffused daylight entered, and saw, what indeed under these circumstances may always be observed, four well-defined coloured bands in the spectrum; red, yellow-green, blue, and violet. The transitions from reddish orange through orange and yellow into green-yellow, from green into blue, and from blue into violet, are so speedy in the flint-glass spectrum, that without the help of a magnifying telescope they entirely escape the eye. In this case the lines G and H of Fraunhofer bound violet very sharply on both sides. The transition from green to blue is marked by the lines b and F, and the narrow strip of pure yellow being, in diffused daylight, very feebly luminous, it recedes in the presence of the stronger red and green, so that these two colours appear immediately contiguous. Wollaston therefore assumes four primitive colours; red, green, blue, and violet.

Thomas Young accepted Wollaston's description of the spectrum, and altered to correspond with it his theory of colours, which first assumed the three primitive colours generally recognized, red, yellow, and blue, in the place of which he now set red, green, and violet; this necessitates the belief that he was aware of the fact that from prismatic red and green yellow may be obtained, and from prismatic green and violet blue. The theory of Young before mentioned is important, inasmuch as in it a definite physiological significance is assigned to the three primitive colours. He assumes that the particles which lie upon the surface of the retina are capable of peculiar vibrations, and that at each place particles exist possessing three different times of vibration corresponding to the velocities of the oscillations of the three primitive colours, violet, green, and red, which are to each other in the ratios of 7, 6, and 5. If the number of vibrations of a luminous ray were 5, it would only act upon the nerves capable of the sensation of red; if the number were $5\frac{1}{2}$, the red

* Philosophical Transactions, 1802, Pt. 2. p. 378.

and green sensations would be simultaneously aroused, and thus the mixed sensation of yellow generated.

I have been equally unsuccessful with Forbes in my efforts to find among Newton's followers, up to the latest period, experiments on the mixture of the single prismatic colours. It appears as if the question was regarded as completely exhausted by the experiments with the mixed powders. Even the divergent results given by the rotating disk were insufficient to convince experimenters that difficulties lay concealed here.

The referring of all colours to the three primitive ones has, in the case of the different observers, three different senses :—

1. That the primitive colours were such as permitted of the formation of all others from their combinations.

2. Or, as supposed by Mayer and Brewster, that the primitive colours correspond to three different kinds of objective light.

3. Or, as supposed by Thomas Young, that they correspond to three primitive modes of sensation experienced by the visual nerves, and from which the remaining sensations of colour are composed.

To the second of these views and the reasons by which Brewster has endeavoured to support it, I will return in another place, and believe that I am in a position to refute it. The two others must, at all events, be tested by the prismatic colours, these being the purest and most saturated that we possess. This shall be the object of the present paper.

The means which I have made use of to obtain the combinations of the colours of the spectrum, two by two, is as follows : I cut in a black screen two sufficiently narrow slits ($\frac{1}{4}$ of a line wide) which together form a V. Both are inclined at an angle of $45°$ to the horizon, the angle which they enclose being thus a right angle which points downwards. This slit is observed from a sufficient distance (12 feet) through a telescope and prism. The prism is placed close before the object-glass of the telescope, in the position of minimum deflection, and the edge of its refracting angle stands vertical. It is known that, looking through a vertical prism at a vertical slit, a rectangular spectrum is observed in which the coloured bands and the lines of Fraunhofer occupy a vertical position. If through a vertical prism an oblique slit be observed, the spectrum assumes the form of an oblique-angled parallelogram, with two opposite sides horizontal and two others parallel to the inclined slit. The bands of colour and the lines of Fraunhofer are here, of course, parallel to the slit. When our compound angular slit is thus observed, the spectra of its two legs partially cover each other, and, as in the one the bands of colour are directed from the left above to the right below, in the other from the right above to the left below, they mutually in-

tersect each other at right angles. Every coloured band of the one intersects in the common field of the spectra each band of the other, and thus we at once obtain the total combinations capable of being formed out of every two simple colours.

As it is necessary to illuminate the slit uniformly through its entire extent, direct sunlight cannot be well applied, and we must content ourselves with the light of the firmament, or of a white surface shone upon by the sun. These lights are in general completely adequate to the purpose.

The flint-glass prism which I made use of, permitted, when direct sunlight and a narrow slit were applied, a great number of the finer Fraunhofer's lines to be seen. In the spectrum of the angular, and somewhat wider slit above described, the stronger lines, at least, were distinctly visible, particularly those which Fraunhofer has distinguished by the letters A, B, D, E, b, F, G, H. The presence of these lines assures us, in the first place, that in the spectrum of each distinct limb of the slit the differently coloured rays could not overlap each other, and hence that we had to deal with pure coloured rays ; and, secondly, they increase greatly the facility of examining the mixed field, through which they are distinctly seen to run. My telescope possessed a pair of cross wires which cut each other at right angles, and these I set parallel to the dark lines of the coincident spectra. The wires thus mark, at the upper and lower rim of the illuminated field, the two simple colours which are mixed together at their point of intersection.

It is necessary to be able to alter the relative intensity of the mixed colours. This I accomplished by bringing the prism from its vertical position into a more or less inclined one. It was so attached to the forward cylindrical end of the telescope as to permit of its being turned round the axis of the latter, and thus might be brought into any required position relative to the horizon. In order to explain how, by this means, the intensity of the light of the spectrum is changed, let us fix our attention upon a single slit. The intensity of the spectrum depends upon the quantity of light which falls from the slit upon the prism and telescope, and on the apparent magnitude of the spectrum to the illumination of which this light is applied. The quantity of light received does not change when the prism is turned round the axis of the telescope, but the illuminated surface of the spectral image changes. The latter, as already remarked, possesses the form of a parallelogram. Two of its sides are parallel to the slit, and always of the same length as the slit appears in the telescope ; the two other sides stand perpendicular to the refracting edge of the prism, and their length depends solely upon the dispersive power of the latter. The spectrum therefore forms

a parallelogram the sides of which are constant, but whose angles can be altered by turning the prism round the axis of the telescope. The known propositions of elementary geometry teach us that the superficial contact of such a parallelogram is a maximum when its angles are right angles, the area decreasing more and more as the obliquity of the angles is increased. Now as the same quantity of light illuminates a smaller surface more brightly than a larger one, the apparent brightness of the spectral image must be a minimum when the image is a rectangle, that is, when the refracting edge is parallel to the slit, and the brightness must increase as the angle enclosed by the slit and the refracting edge increases.

The two legs of our angular slit, when looked at through a vertical prism, give two spectra equally bright, inasmuch as the refracting edge is inclined towards each at an angle of 45°; when however the prism is turned round the axis of the telescope, one of the angles becomes greater and the other smaller, the relative brightness of the spectra being thus caused to vary in any required degree.

The brighter a spectrum is made in this way, the more closely are its coloured bands pressed together: lest this should too much prejudice the spectrum's purity, it is advisable to obtain great differences of brightness in another way than that just indicated. This is accomplished with great facility by placing pieces of paper, oiled or not oiled, of greater or less thickness, behind one of the slits. These permit only a small portion of the incident light to shine through, while through the other slit passes the unenfeebled light of heaven.

When a field is obtained in the manner described, covered with the mixtures of every two pure colours of the spectrum, the observer readily convinces himself that the hues, particularly those in the whiter portions of the field, cannot be estimated while saturated colours stand beside them.

It is therefore absolutely necessary to separate the portions regarding whose colour we wish to form a judgement, from the remaining ones. When the telescope is used in the observations, the means of effecting this is very simple. Let the cross wires be fixed upon the place in question, and let the observer recede to a distance of one or two feet from the eye-end of the instrument. From this distance only a small portion of the spectrum is visible through the eye-glass, the colour of which may be estimated apart from the disturbing influence of the dazzling colours adjacent. If the observer be long-sighted enough, the intersection of the cross wires is seen from this distance with the naked eye, at all events by aid of a weak concave glass suited to the eye. In order to rediscover the observed combinations with

dispatch, and to show them to others, I place at the above distance from the eye-glass a dark moveable screen, with a small round orifice, through which the observer looks towards the eye-glass of the instrument. If it be wished, instead of the compound colour, to see the two composing simple ones, one slit after the other may be closed by a second person, so that only one of the two mixed colours remains standing; or a second small prism is introduced between the eye and the orifice; and thus, instead of a single bright spot in the eye-glass of the tele-cope, two with distinct colours are observed. For the more certain determination of mixed colours which approach very nearly to white, it is useful to encircle the opening of the eye-piece with a sheet of white paper illuminated by white light, and to compare its colour with the observed one. I have also noticed that the eye is rendered less sensitive to fine differences of colour by long gazing upon very whitish mixed colours; and it is therefore advisable to permit the eye to rest at times, or to allow it to wander over the surrounding objects. When the ob-servation is renewed, a mixture of colour is often plainly detected in the apparent white which had previously escaped observation, and which, when long looked at, again disappears.

In this way it is possible to obtain the total combination of every two of the simple prismatic rays in all degrees of relative strength, and to observe them undisturbed by the presence of other colours. My observations, the principal points of which I have had corroborated by the testimony of several other persons practised in the judgement of colours, thus avoiding whatever error the subjective defects of my own eyes might occasion, have furnished the following results, some of which differ, in a sur-prising manner, from the views on this subject heretofore held.

1. *Red* gives with orange a redder orange; with yellow, orange: the mixed colours do not differ sensibly from the de-grees of orange which appear in the simple spectrum. With green it gives a yellow, which, less saturated, is paler than the simple yellow, and which, when red is predominant, passes through orange into red, and when green is predominant, passes through yellow-green into green. With the green-blue tones of the spectrum, a flesh-colour is obtained; with the sky-blue ones, a rose-red colour, which, when blue predominates, passes into whitish violet, but when red predominates, passes into carmine-red. When, finally, the red is combined with the indigo and violet rays, which lie further towards the end of the spectrum, a purple-red of increasing depth and saturation is obtained.

2. *Orange* gives with yellow a yellower orange; with green, a pale yellow; with blue, flesh-coloured tones, which, with indigo and violet, pass over into carmine-red.

3. *Yellow* with green gives a greenish yellow, similar to the tones which lie between these colours in the spectrum. With sky-blue it gives a weak greenish white; with indigo-blue, *pure white*; with violet, a weak flesh-coloured white, which, when violet predominates, passes over into whitish violet, and when yellow predominates, into a whitish yellow.

4. *Green* gives with blue, green-blue; with indigo, a bright blue, which however is much duller and whiter than that of the spectrum; with violet also it gives a bright blue.

5. *Blue* with indigo gives the tones of colour which lie between them; with violet, a dark blue, which however is less saturated than the indigo of the spectrum.

6. *Indigo* with violet gives the intervening tones.

The most surprising fact, and that which deviates most from the views hitherto entertained, is, that of the colours of the spectrum there are only two which together give pure white, that is, which are complementary to each other. These are yellow and indigo-blue, two colours from the combination of which it has been hitherto invariably imagined green would result. The yellow which is made use of in this mixture is a very narrow band in the spectrum, lying between the lines D, E, and about three times more distant from E than from D,—a yellow which approximates neither to orange nor to green, and among the pigments is best represented by chromate of lead (chrome-yellow). The blue made use of with this has a greater width, and embraces the degrees of this colour distinguished by Newton and Fraunhofer as indigo, from about the centre between F and G almost to G. Among pigments, dark ultramarine represents this colour better than the more violet indigo. When the colours to be mixed are obtained from two equally bright spectra of a flint-glass prism, the light used being that of the clouds, then the exact centre between the lines F and G is the point which possesses the proper luminous intensity for the production of white. Towards the violet and the line G the blue becomes more feebly luminous, and hence it must here be strengthened in comparison to yellow, in order that white may be obtained. For this reason, in the spectrum of a whitish blue firmament, for example, the white falls near the line G. The brighter blue of the spectrum, which approaches nearer to the line F, gives with pure yellow, by the proper arrangement of their relative intensities, tones which are very similar to white, but which nevertheless are not without a feeble colouring. The same remark applies to violet when mixed with a greenish yellow. The tint approximates, in most cases, to flesh-colour, or to a bluish and greenish hue; it is, however, sometimes difficult to assign a distinct name to the tint; but I have never succeeded in obtaining

from these colours a clear pure white. If the investigation were conducted with instruments more complete than those applied by me, and which would permit of the formation of a larger field of compound colours, the limits of those rays which produce white would probably be estimated with greater accuracy, inasmuch as the comparison of the hues of large surfaces is capable of being effected with much more ease and sharpness.

By the rays which produce white, the whole width of the spectrum is divided into three sections. The first of these corresponds to the red, and, if we compare the ratio of the luminous vibrations with those of sonorous waves, answers to about the interval of a small third, the middle green section to a great third, the third and violet section being somewhat smaller than a small third. Colours of the first and second sections combine to tones of yellow, with transitions into red, flesh-colour, white, and green; those of the second and third combine to blue, with transitions into green, white, and violet; those of the first and third combine to purple-red, with transitions into flesh-colours, rose, and violet.

With respect to the combination of three simple colours, we may conclude that white can only be the result when rays from the three different sections of the spectrum are suitably united. At least it cannot be supposed, although all possible combinations cannot, of course, be exhausted by experiment, that the yellow or yellowish colours, for example, which are derived from the red and green sections, can, by the addition of one or more colours contained in these sections, red, yellow, or green, pass over into white. This remark is also applicable to the mixtures of the green and violet, as also to those of the red and violet sections. We may, on the contrary, succeed in obtaining white from various combinations of three colours taken simultaneously from the three sections. For this purpose I have made use of a black screen with three slits. Two of these were parallel and inclined at an angle of 45° to the horizon; they stood at such a distance from each other, that, when observed through the prism at the ordinary distance, the violet of the one fell upon the red of the other. The slit from which the violet is obtained must be made about twice as wide as the other, for otherwise the violet is too feeble in comparison with the red. A third slit, which is to yield green for the mixture, was cut between the two others and at right angles to them, so that the three slits together presented a figure similar to a Z. The spectrum of the third slit intersects at right angles the purple stripe given by the two others, and generates a series of mixed colours in which the whitest portion is easily sought out. By turning the prism round the axis of the telescope, the mixed colours can be so

made to balance each other that pure white is procured.   Thus we obtain white from red, green, and violet, which may be combined to three pairs of complementary colours; namely,

Simple red and compound dull blue-green.
Simple green and compound purple-red.
Simple violet and compound dull yellow.

It is a striking fact, that, while the complementary colours of simple red and violet are only distinguished from certain tones of the spectrum by their less saturated appearance, the former nevertheless give, with simple red and violet, white, the latter not.

Newton's few observations on the combinations of every two prismatic colours coincide with my results.   He finds that the primitive colours can be obtained by the combination of the neighbouring ones at both sides of the former; for example, orange can be formed from red and yellow; yellow, from orange and green-yellow; green, from green-yellow and sea-green, and also, but not so good, from yellow and blue (*cyaneum*); blue, from sea-green and indigo.   He has also formed from red and violet, purple-red.   White he could only obtain from the three colours, red, violet, and green; and in order to render the experiment successful, he even recommended the application of spectra whose colours were not completely separated.   In this case more than three single colours are mixed together.

It will, on the contrary, be observed, that my results on the action of prismatic colours differ materially from those obtained by the mixing of colouring substances.   In particular, that yellow and blue do not furnish green, but at most a weak greenish white, contradicts in the most decided manner the experience of all painters during the last thousand years.   The reason of the contradiction will, however, be rendered quite plain by reflecting a little upon the manner in which colouring substances act upon light.   The substances used in painting, as all coloured bodies of regular structure which we possess in large pieces, for example, crystallized cinnabar, crystallized chromate of lead, cobalt-glass, from which smalt colours are made, are transparent, or at least translucent.   When light falls upon them, a portion of the latter will be reflected from the exterior surface as white light; another portion enters the substance, and by the unequal absorption of the component simple rays becomes coloured, is reflected at the posterior limiting surface of the body, and returns to the eye of the observer, which, by means of this particular portion of light which has entered the body and been reflected within it, sees the latter coloured.   When, however, we grind a colouring substance to powder, the observer sees not only that portion of the incident light which is reflected at the forward and poste-

rior surfaces of the uppermost layer of powder-particles, but also that reflected from the second, third, fourth, &c. Of light perpendicularly incident, a single glass plate reflects only $\frac{1}{25}$, two such plates only $\frac{1}{13}$, very many plates almost the whole. We can conclude from this, that of the light which falls upon the fine white powdered glass, the smallest portion only is reflected from the uppermost particles, a much greater portion being reflected by those beneath. This must also be the case with coloured powders, at least with those simple rays whose colour they bear, and which are permitted to pass without absorption; the greater portion of light of this kind comes from the deeper layers, having traversed in their passage a number of powdered particles.

Let us consider what will be the case when we mix together powders of different colours, for example, yellow and blue. The blue particles which lie upon the surface will give blue light, the yellow which lie upon the surface will give yellow light; both together will combine to form white or greenish white. It is quite otherwise, however, with the light which returns from greater depths. This must pass through yellow and blue particles, and hence from a distance below the surface such light only will return as can penetrate both the yellow particles and the blue ones. Blue substances generally permit green, blue, and violet light to pass through them in sensible quantity; yellow, on the contrary, permits red, yellow, and green to pass. Green, therefore, is the only light which will pass through both, and hence from the deeper layers of the mixed powder only green light can return. Now, as the quantity of light reflected from the superficial portions of the powder is, according to what has been already said, generally much smaller than that which returns from the deeper layers, the consequence is, that the green of the latter is by far the most predominant, and thus determines the colour of the mixture.

When therefore to a blue powder we add a yellow one, the colour of the mixture is less altered by the addition of the yellow rays to the blue, than by the circumstance that of the latter rays the violet and blue portions are lost, and the green alone remains. For this reason also mixtures of two colouring substances of nearly equal intensities are in general darker than their constituents, especially when the latter possess such colours as stand far apart in the prismatic series, and hence contain but few rays of a common nature. Thus cinnabar and ultramarine, instead of the rose colour, which corresponds to the composition of their rays, give a black-gray which approximates somewhat to violet.

The theory of pigmentary colours here presented is simply derived from the generally recognized laws of physics; it explains

the phænomena, so far as I am able to see, completely; showing that the mixture of the substances and the combination of their colours are two processes altogether distinct, and hence that the results obtained from the former furnish no conclusion regarding the latter. Only when we have to deal with colours which stand but slightly separated from each other in the spectrum does the composition of the coloured light give nearly the same results as the mixture of the pigments, for then the compound colour is similar to the tones of the spectrum which lie between both the simple ones.

There are, however, two other methods of combining the light proceeding from pigments, which yield results altogether in harmony with those obtained from the combination of similar prismatic colours. The first of these methods is the union of the colours upon the rotating disc. It has been long noticed, that results thus obtained are different from those derived from the mixture of the pigments. I repeated the experiment with yellow and blue. For the former I either made use of gamboge or chrome-yellow, for the latter ultramarine or mountain-blue. With quick rotation a pure gray is obtained. The difference of the two methods is exhibited very strikingly when the middle of the disc is coated with a mixture of both pigments, while the rim is divided into sectors coloured by the pure pigments themselves. With quick rotation the middle of the disk appears green, the rim gray. The former is much darker than the latter, which according to the foregoing theory must be expected.

Of the other method I have never yet found a description, but can recommend it as very convenient. It is free from the defect of the gray appearance of the mixed colours which is observed upon the rotating disk, and admits, on the contrary, of the generation of a perfect white from complementary-coloured pigments. Let a glass plate, with plane and parallel surfaces, be placed perpendicular to the leaf of a table, and let a coloured wafer be placed before it. The image of the wafer is reflected by the glass plate; the apparent place of the image is at the other side of the plate, and also on the surface of the table. Let another wafer of a different colour be placed upon the exact spot where the image is observed, this second wafer being seen through the glass. The observer's eye will thus be affected by two descriptions of rays, both of which appear to proceed from one and the same body, one of which however belongs to the transmitted and the other to the reflected light. Hence he observes a wafer the colour of which is compounded of those of the two wafers actually before him. To make the experiment with greater convenience, it is only necessary to use a very small glass plate, as thin as possible, and with plane parallel surfaces; this is to be

fixed at right angles to the table at about the distance of distinct vision. The observer looks obliquely through the plate downwards towards the table, and places the wafers in a position which is suitable for the combination of their colours. The nearer both are brought to the imaginary intersection of the plane of the table with the glass plate, the more obliquely will the rays fall upon the plate, the fewer will pass through, and the greater will be the number reflected; so that, in this case, the colour of the reflected light will be predominant. Conversely, the colour of the transmitted light will be predominant when the wafers are removed to a distance from the line of intersection; and in this way it is possible to alter the relative intensities of the combining colours in any required degree. In this experiment both wafers are placed upon a black ground*; or if whitish combinations of colour be required, which it is necessary to compare with pure white, one of the wafers (the brightest is best) is placed upon a white, the other upon a dark ground. Observed through the glass plate, the wafer appears in the compounded colour upon a white ground. It is manifest that in this way the colours of all coloured surfaces whatever, as also those of coloured glasses, may be combined.

Colours thus composed are distinguished by their brightness and clearness from those obtained by the mixture of the colouring matters; they do not always agree with the latter, but, on the contrary, yield the same results as those obtained when the prismatic colours are united. Blue and yellow, in particular, do not give green, but white. As the representative of the yellow, I made use of paper disks which I had washed with bright chrome-yellow or gamboge. Of blue colouring matters laid on disks in the same manner, a beautiful sky-blue cobalt gave, with both kinds of yellow, pure white; artificial ultramarine, reddish white; and bright Prussian blue, a weak greenish white. Cinnabar combined with blue gives rose-colour; the same red colour combined with green gives yellow, &c. In short, these experiments prove, that not only the simple coloured rays of the spectrum have other laws of action than those hitherto generally assumed, but that quite similar laws apply to the combination of the colours of pigments. It does not appear to me doubtful that these new laws will supersede the old ones which were based upon the mixture of the colouring substances.

It is best, however, to commence with the simple colours of

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* I have repeated the experiment with a single yellow wafer on a blue ground, the image of the wafer projected upon the latter gives a *white* spot.—J. T.

the solar spectrum, because these are the purest and most perfect, and even with a moderate intensity of light make an almost dazzling impression; beside which, all pigmentary colours appear dull and gray. Newton has already given the rule, that each simple colour can be obtained from the union of the two next it. My own investigations corroborate this. I must, however, at the same time remark, that the distance of the combined colours must not be too great, when it is sought to obtain from their union a colour similar to that which lies between them. This is particularly the case in the central portion of the spectrum. Red and yellow give an orange, the appearance of which appears to be quite the same as that of the simple orange; and, in like manner, the indigo which results from the combination of blue and violet is scarcely to be distinguished from the simple indigo. On the contrary, yellow-green and blue-green give a green the tone of which indeed corresponds to the intervening tone of the spectrum, but which is decidedly duller and more whitish, so that the simple green can be obtained from such colours only as scarcely differ from it in appearance. Yellow and blue appear in this respect less sensitive than green. The former may be pretty well obtained from orange and yellow-green, but very pale from red and green; the latter, again, may be well obtained from the combination of blue-green and indigo, but is very dull when formed of green and violet. With regard to the end colours of the spectrum, red and violet, Newton in his coloured disk places them in contact with each other, and subjects them to the rule which refers to the union of neighbouring colours mentioned above. From indigo-blue and very little red it is indeed possible to generate a kind of violet, which however always approximates more to white or rose-colour than the simple violet. Much more incomplete appears to my eyes the imitation of red by orange and violet; their combinations always pass into tones of carmine-red or of white, and I have not succeeded in obtaining a tolerable imitation of the pure red of the spectrum.

Hence if we propose to ourselves the problem of imitating the colours of the spectrum by the union of the smallest possible number of simple colours, we find at least five of the latter necessary for this purpose, namely red, yellow, green, blue, violet. I must, however, leave the question undecided, whether these are completely sufficient, and whether with better apparatus, which would permit of the illumination of larger surfaces by the simple colours, and by the corresponding compound ones placed adjacent, a practised eye might not detect differences which with my apparatus could not be recognized. If, however, we wish to limit ourselves to three colours, it would be best to

choose the three simple ones which admit of the least perfect imitation, namely red, green, and violet : we should then obtain a yellow and blue, which, when compared with the colours of our pigments, would appear saturated, but which would not bear comparison with the yellow and blue of the spectrum. These are the three which Thomas Young proposed as the three primitive colours. Red, green, and blue would not answer so well ; for were these three chosen, the mixed violet would appear worse than the mixed blue of the former three. The three primitive colours commonly chosen are altogether insufficient, because from them green can never be obtained.

According to the above we must also abandon the theory of three primitive colours, which, according to Thomas Young, are three fundamental qualities of sensation. If the sensation of yellow by the yellow rays of the spectrum were due to the fact that by them the sensations of red and green were simultaneously excited, and both working together produced yellow, exactly the same sensation must be excited by the simultaneous action of the red and green rays ; nevertheless by the latter we can never obtain so bright and vivid a yellow as that produced by the yellow rays. The same remarks apply to blue, which would be formed from the mixture of green and violet ; and to violet, which would be formed from the mixture of blue and red. To retain in this sense the theory of primitive colours, five such, at least, must be assumed. On the contrary, to represent and classify the dull and comparatively impure colours of natural bodies, in the sense of Lambert and Forbes, three primitive colours would be quite sufficient. But, for a sure and a scientific classification, it would be necessary to apply a method of combining colours different from the mixing of pigments.

By the union of every two simple colours we are met by two new impressions, namely white and purple-red, with their degrees of transition into the simple colours before named. The purple-red belongs to the saturated colours which cannot be otherwise obtained than from the extreme red and violet, without a loss of brightness. White, on the contrary, can be obtained in an infinity of ways, without the eye being able to distinguish one white from the other. We obtain it for example from simple yellow and blue, from simple red, green, and violet, or from these five simple colours taken together ; and besides these, from several more complicated combinations. In contrast with colours it is therefore regarded as indifferent light. The remaining combinations of every two simple colours appear to the eye as transitions of the simple colours and purple into white ; but in further combinations, as above remarked, they behave in a

manner essentially different from the colours of the spectrum when the latter are weakened by the addition of white light.

In conclusion I give the following small table; it furnishes a general view of the combinations of every two colours, and in its construction five colours are assumed, by the union of which the colours of the spectrum are represented with sufficient accuracy.   In the first horizontal and the first vertical series stand the simple colours; the compound colours which follow from their union are found at the intersection of the corresponding horizontal and vertical columns.

	Violet	Blue	Green	Yellow	Red
Red	Purple	Rose	Dull-yellow	Orange	Red
Yellow	Rose	White	Yellow-green	Yellow	
Green	Pale-blue	Blue-green	Green		
Blue	Indigo	Blue			
Violet	Violet				

LXXXII. *Description of a new Evaporating Gauge.*
By Mr. JOHN NEWMAN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE for many years noticed that the meteorological journals kept by scientific observers omit all mention of the quantity of water evaporated from the earth's surface.   This omission has, I believe, arisen from the want of a correct instrument, or one sufficiently delicate in its indications.

After much consideration, I beg to hand you for insertion in your Journal, the description of an instrument which I believe will be found efficient, adding to the completeness and usefulness of the meteorological notices of those who are pleased to use it.

It consists, as you will observe (from the accompanying sketch), of a short cylinder 12 inches in diameter, having connected with it, by means of a stopcock, a glass tube graduated to hundredths, and terminating in a lower vessel, which will con-

tain a sufficient quantity of water to be raised by artificial pressure into the upper one for exposure to the atmosphere.

To use the apparatus, pour water into it until it rises to the zero in the glass tube, then by means of a syringe force air through the tube × into the lower vessel, so as to raise the water into the upper one to any height you please. Now shut off the stopcock beneath to retain the water in the upper vessel; then, having exposed the apparatus for any length of time that is required, open the cock, when the water will run into the lower vessel, filling it and part of the glass tube, the divisions of which will now indicate in hundredths the quantity of water evaporated.
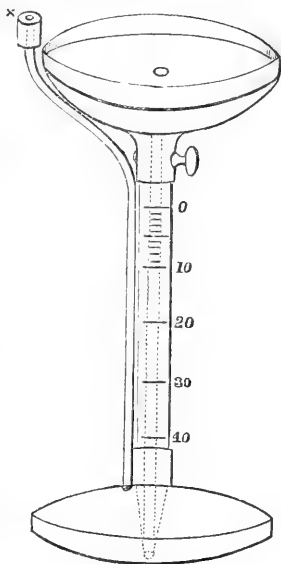
I am, Gentlemen,

Your obedient Servant,

122 Regent Street,   JOHN NEWMAN.
London, Nov. 10, 1852.

LXXXIII. *Notices respecting New Books.*

Life Contingency Tables.——Part I. The Chances of Premature Death and the Value of Selection among Assured Lives. By EDWIN JAMES FARREN, *Fellow of the Institute of Actuaries, and one of the Actuaries authorized to certify Tables for Friendly Societies.*

THE continual increase which has been, for many years past, taking place, more particularly in this country, in the amount of property either assured upon human life, or in some way or other depending upon it, has rendered the accurate determination of its average duration a matter of the utmost importance; and accordingly many most laborious researches have from time to time been entered upon, with a view to ascertain not only the true rate of mortality prevailing amongst the general population, but also that found to obtain in particular localities, or under circumstances not of an ordinary character.

In the very able work before us, Mr. Farren has proposed to himself the resolution of a problem looked at with much interest by

persons familiar with this subject; availing himself of certain data collected by competent persons, with great care and precision, from the records of several of the principal assurance companies, he has endeavoured to show the effect of selection amongst assured lives, that is, to what extent the rate of mortality is diminished during a given year amongst persons of all ages pronounced to be eligible for assurance at the commencement of that year. A table formed from such observations might, as Mr. Farren says, "be expected to display the extreme value of selection, and thus afford a minimum rate of mortality;" for it must be remembered, that, in the ordinary tables, the numbers exposed to the chance of death at every age are made up of persons selected at that and almost all previous ages. Compared with such tables, and with one exhibiting the rate of mortality prevailing amongst the male population of England and Wales, the results obtained by Mr. Farren are very curious. We give the following brief abstract to enable the reader to judge of them :—

Ages.	Mr. Farren's results. Mortality per 1000.	General mortality amongst assured lives. Mortality per 1000.	Mortality amongst the male population. Mortality per 1000.
21 to 30	6·2214	8·1342	9·1347
31 to 40	7·4866	9·7320	11·7947
41 to 50	11·4201	13·3971	15·2106
51 to 60	21·3524	23·4302	22·9492
61 to 70	41·4718	46·7017	48·8562

It will here be seen, that selection operates at all ages with considerable force in diminution of the ordinary mortality, and that, as might be expected, the force has a tendency to increase as the age increases. It is probable that the general effect would be still greater, but that it is counteracted by the efforts continually made to effect assurances on not very good lives, "the more precarious the life the greater the inducement, as Mr. Milne observes, for persons interested in its continuance to get it insured." One remarkable feature in the tables is the comparatively low rate of mortality amongst the general population in the last decade but one, that obtaining amongst assured lives being actually higher. Mr. Farren accounts for this, partly by a peculiarity in the construction of Mr. Farr's table (the mortality amongst the male population), and partly by the supposition, "that persons seeking life assurance at the ages in question form a better criterion of their own health than at other periods of life, and thus render the task of selection by parties having an opposite interest more than usually difficult." It will probably be conceded, that about the period of life referred to, intimations of latent diseases are of more general occurrence than at any earlier one, and that in later life their development makes them apparent; but these considerations scarcely seem to suffice for explanation of the anomaly.

We have not space to follow Mr. Farren through many other ingenious deductions, but we must not pass by without comment the

learning and ability displayed by him in the adoption of methods for the construction of his tables. After exhibiting various modifications of the function a^{b^c}, first applied by Mr. Gompertz to these investigations, Mr. Farren makes use of the calculus of finite differences for the purposes of interpolation till the age of seventy-one. After that age, because " the admissions are too few to admit of the supposition that the same mixed generic law will continue to prevail," he has recourse to the formula $\lambda^x l = \lambda' l \dfrac{r^x - 1}{r - 1}$, by means of which, and the theory of equations of n dimensions, the remainder of the table is interpolated.

Lastly, the means are afforded of tracing, from the original data, the several values made use of; so that the whole series of operations may be followed, and the connexion between the first and last values easily confirmed. As Mr. Farren wisely observes, " all modern publications on these subjects must principally depend, for even ordinary acceptance, upon the attention paid to such details ;" nothing, he may be assured, was better calculated to secure for his labours the confidence they so evidently deserve. He is quite right, we think, when he says that tables of mortality solely depending for their adoption on the authority of the authors framing them, will eventually be discarded.

Brief Astronomical Tables for the Expeditious Calculation of Eclipses in all ages. By W. Drew Snooke. S. Highley and Son, Fleet Street.

These tables are constructed with the view to enable the historical student to verify dates by the calculation of eclipses named in history. For this purpose strict mathematical accuracy may be dispensed with; and the smaller equations which enter into the determination of the times of full and new moon, and the relative position of the three bodies, may with more or less risk of error be omitted from the calculation. The tables are well arranged, and the rules for using them clearly expressed and exemplified.

The author appears to handle his subject with familiarity; and if the correctness of the tables may be relied upon, and the time and other circumstances of the phænomena to which they are applicable is capable of being educed from them with that degree of accuracy which the author affirms, we think his little book may be found useful in supplying a desideratum which is likely to have been felt by many of the class of students for whose use it is intended.

Preparing for Publication.

An Elementary Introduction to the Study of Palæontology; with numerous Figures Illustrative of Structural Details. By F. M'Coy, Professor of Geology and Mineralogy, Queen's College, Belfast.

Also, by the same Author,

A Manual of the Genera of British Fossils; comprising Systematic

Descriptions of all the Classes, Orders, Families, and Genera of Fossil Animals, found in the Strata of the British Isles; to be completed in four or five Parts, forming one volume, 8vo, of about 500 pages, with nearly 1000 Wood Engravings.

LXXXIV. *Intelligence and Miscellaneous Articles.*

ON A REMARKABLE DEPOSIT OF TIN-ORE AT THE PROVIDENCE MINES NEAR ST. IVES, CORNWALL. BY WILLIAM JORY HEN-WOOD, F.R.S., F.G.S.*

THE Providence Mines, in the parish of Lelant, comprise the mines formerly known as Wheal Speed, Wheal Laity, Wheal Comfort, and Wheal Providence, long worked on the eastern side of the hill which slopes from Knill's monument to the sea.

(*a.*) Observations on the eastern workings in the slate, and on the western within the granite formation, have already appeared in the Royal Cornwall Geological Society's Transactions †. The intermediate tract now to be described is wholly in granite, of which the upper beds are composed of a basis of grayish felspar and quartz, imbedding medium-sized crystals of white felspar, as well as numerous small groups of schorl in radiating crystals: but near the productive parts of the *lodes* the rock is mostly rather coarse-grained, its basis is greenish-gray felspar, black mica, and quartz; and the included porphyritic crystals of felspar are either of a pale buff, a pink, or a reddish-brown hue.

(*b.*) The veins are :—

The Cross-Course or Trawn, which bears about 22° W. of N., and dips E.‡		
The Wheal Comfort lode ... ... 15° W. of N., ... W.		
and Wheal Laity lode or lodes ... 17° S. of W., ... S.		

Connected with the Wheal Comfort lode there is a "Carbona§," to which further reference will be made presently.

It may be here stated generally, that the Cross-course is from one foot and a half to two feet in breadth, and is composed of disintegrated fine-grained granite, divided by numerous joints parallel to the "*walls*;" as well as by many other curved and irregular ones which intersect each other in every imaginable manner, and are filled with oxide of iron, and closely but unconformably striated.

The Wheal Comfort lode varies in width from a few inches to more than six feet. At a distance from the Wheal Laity lodes it is of granite very thinly impregnated with tin-ore; the remainder consists of quartz, schorl-rock (*capel*), brown iron-ore, and greenish

* From the Transactions of the Royal Geological Society of Cornwall, vol. vii.

† Vol. v. pp. 16–20, plate 2. fig. 7, Tables 21 and 22.

‡ The "directions" have reference to true north, the "dips" are from the horizon.

§ I have already described a similar, though a much smaller formation, in one of these mines.—Corn. Geol. Trans. vol. v. Table 22.

and brownish felspar, in some places—near the Wheal Laity lodes—abounding in tin-ore.

At about 105 fathoms deep this *lode* is connected with one of those curious deposits of tin-ore locally called "Carbonas *," as yet unknown in any other part of Cornwall. The union takes place about 14 fathoms south of the contact between the Wheal Comfort and the Wheal Laity lodes; and for 10 fathoms above and 20 fathoms below, as well as for the whole distance between the Wheal Laity lodes and the Carbona, the Wheal Comfort lode when alone is very productive; but immediately when the Wheal Comfort lode and the "Carbona" separate in descending,—each taking its own downward course,—the *lode* becomes unproductive, and so also remains as far southward as it has yet been traced.

At the northern contact of the Wheal Comfort lode and the "Carbona" there is a rich mass of quartz, felspar, schorl, and tin-ore, at least 15 feet in width for about 5 fathoms in length: both southward of and below this spot the *lode* preserves its usual direction and dip; but the "Carbona" southward bears about 5° east of the course of the *lode,* and holds nearly perpendicularly downward. Descending about 5 fathoms it abuts on the granite rock, and is seen no deeper; except that as it is pursued southward, the irregular granitic bed on which it rests declines at an angle of about 8°. With the exception of a single short string or pipe, no trace whatever of the "Carbona" has rewarded the numerous researches which have been made at greater depths. Nothing can, however, be more irregular than its size and various ramifications. Though the upper edge of the "Carbona" generally continues to touch the lower side (*foot-wall*) of the *lode,* in some places the contact is only a few inches, but in others as much as two fathoms and a half wide. Again, in some cases the continuity of the "Carbona" where it joins the *lode,* is almost entirely cut off by intervening masses of granite; the union with the main body being still preserved, though merely by "pipes" or "pillars" of *lode*-like matter. Many portions of the "Carbona" are as much as five or six fathoms high, others not more than four or five feet; some parts are two fathoms and a half wide, whilst others do not exceed six inches. The largest portions are, however, seldom or never entirely separated from each other by the containing rock, for there is always a sufficient connexion to conduct the miner from one large and rich mass to another.

The composition of the Wheal Comfort lode has been already noticed; but notwithstanding their intimate connexion, that of the "Carbona" is widely different, as its tin-ore occurs chiefly in quartz and schorl, which minerals, either separate or mixed, constitute the far greater portion of this remarkable deposit.

* Some persons pretend to derive this term from the ancient Cornish language, whilst others suppose it to have been recently *coined* by the miners. Both the word itself and the metalliferous deposit it is meant to designate are, I believe, confined to the St. Ives mining district.—Corn. Geol. Trans. vol. v. p. 21, note.

Everywhere eastward the Wheal Laity lode is but a single vein, of about a foot and a half wide, and composed of quartz, earthy brown iron-ore, greenish, and in some places brick-red felspar, a little tin-ore, together with some vitreous copper-ore and iron pyrites. Westward, however, it consists of at least two separate veins, called for distinction sake the Wheal Laity north and south lodes; and sometimes there is also a third vein. At one spot the third vein is simply crystallized felspar, and the axes of the crystals are parallel to each other, but lie across the vein; in other parts it is slightly productive of tin-ore. The Wheal Laity north, and Wheal Laity south lodes, in general from a foot to a foot and a half in width, are occasionally much wider. Greenish felspar, quartz, schorl, and occasionally brown iron-ore, are their chief ingredients: in some parts both veins are rich in tin-ore; vitreous copper ore, copper and iron pyrites also occur, but are not common constituents. In the deepest part of the mine (*i. e.* at 150 fathoms deep) the Wheal Laity north lode is for some fathoms in length about two feet in width, and is then composed of chlorite, vitreous copper ore, and iron pyrites, and has a vein of rather fine-grained granite on one side. At a depth of 120 fathoms, and about 60 fathoms west of the portions already described, where the same *lode* consists of granite, quartz, red iron-ore, and a little tin-ore, there is connected with its northern side (*foot-wall*) an offshoot or excrescence about 4 fathoms in all directions, but most irregular in figure, and having many small vein-like branches. This mass, consisting chiefly of chlorite, quartz, and iron pyrites, is not only far richer in tin-ore than the adjoining portion of the *lode*, but is remarkably different in mineral composition. We have thus the same ore richly impregnating, not only the Wheal Comfort lode and the "Carbona," two parallel but entirely dissimilar deposits, but also the Wheal Laity lode, which has a direction nearly at right angles to them.

(*c.*) The intersections of the lodes just mentioned exhibit almost an epitome of that class of phænomena.

(1.) The Wheal Laity and the Wheal Comfort lodes cross each other; still at some levels there is no evidence to show that either is cut through; whilst at others the Wheal Comfort lode not only intersects, but also *heaves* the Wheal Laity lode. It is not the least remarkable circumstance attending this intersection that the Wheal Laity lode is a single vein everywhere eastward of the Wheal Comfort lode, whereas westward of their contact it is divided into two, and in some places even into three distinct and separate veins.

(2.) All these veins are intersected by the Cross-course, and all are *heaved* by it; the two larger (the Wheal Laity north and the Wheal Laity south lodes) in general from 10 to 15 fathoms; the displacement of the smaller vein is, however, much less considerable, and does not exceed six fathoms and a half.

Again, notwithstanding the Wheal Comfort lode and the Cross-course have opposite inclinations, they respectively *heave* the Wheal Laity lodes in the same direction.

At a depth of 110 fathoms, where the Wheal Laity north lode is

for some distance unproductive, whilst the Wheal Laity south lode is rich in tin-ore on both sides of the Cross-course, and for some fathoms both above and below the gallery (*level*), the Cross-course consists of a rich vein of tin-ore for the whole interval (five fathoms) between the eastern portions of the two *lodes*, as well as of a fine mass of the same ore at its contact with the western part of the Wheal Laity south lode.

(3.) At 130 fathoms deep the Wheal Laity south lode is also *heaved*, but in an opposite direction, by a vein of granitic clay (the *Flucan*). This *flucan* is not prolonged to either of the other Wheal Laity veins; nor, indeed, does it reach any other gallery (*level*) even on the same *lode*.

(4.) The Wheal Comfort lode and the Cross-course have the same direction, but, as already observed, opposite inclinations; and are so situated that they come into contact on the line of their dips at about 130 fathoms deep. From the point where they first touch each other they descend perpendicularly side by side for about three fathoms, each keeping the same relative position it had previously when separate (viz. the Cross-course on the west, and the Wheal Comfort lode on the east). At length, however, the *lode* cuts through the Cross-course. After this intersection, though they have changed sides, and their relative position is reversed, they still proceed together, but now take the line of the *lode's* previous underlie for several fathoms. When they separate, the lode preserves its dip; but the Cross-course, though it resumes the previous direction of its inclination, dips eastward far more rapidly than before. It may, indeed, be generally observed, that a vein which has been displaced by another, whether the intersection be horizontal or vertical, makes (if I may be permitted the expression) an effort to resume its original course.

(5.) The Wheal Laity lodes are intersected as well by the Wheal Comfort lode and the Cross-course during their union, as by each of them when separate; the union, however, has little or no influence on the extent of the heave.

Many details of local, and some, indeed, of general interest, scarcely need be mentioned, as this paper may be deemed supplementary to my remarks on the St. Ives District[*]; and particularly to a description of a similar interesting formation at the St. Ives Consolidated Mines, which has already appeared in the Transactions of the Royal Geological Society of Cornwall[†].

A small stream issues from the Wheal Laity north lode at 150 fathoms deep, having a temperature of 71°; whilst that of the water discharged by the pump at the adit (45 fathoms from the surface) is only 63° 6′[‡].

[*] Corn. Geol. Trans. vol. v. p. 16.   [†] Ibid. p. 21.
[‡] Observations on the temperature of other parts of the Providence Mines are recorded in the Society's Transactions, vol. v. p. 390.

ON THE NATURE AND NAME OF OZONE.	BY C. F. SCHÖNBEIN.

MM. Ed. Becquerel and Fremy* have recently confirmed the observation, already made by others, that ozone may be produced in the purest possible oxygen when this is subjected to the influence of electrical discharges. These physicists are on this account of opinion that ozone is to be regarded as allotropic oxygen, and propose to call it " *Oxygène électrisé.*" I consider this term inappropriate, and upon the following grounds :—

1. Oxygen may be ozonized not only by electricity, but also by ponderable substances, as for instance phosphorus, or in other words, may be so influenced that it will effect oxidations even at ordinary temperature which would not otherwise take place. According to my opinion there is no disengagement of electricity during the modification of oxygen by means of phosphorus, the oxygenization of turpentine, &c., whence it appears to me to follow that the formation of ozone, through chemical action, is unconnected with electrical action, at least directly. I have recently endeavoured to show that under suitable circumstances 1000 grms. of phosphorus convert 1720 grms. of oxygen into ozone, and indeed with tolerable rapidity. In order to ozonize this quantity of oxygen by means of electrical discharges, the electricity of a thunder-storm would probably be necessary ; for even the most powerful discharges which we are able to pass through oxygen or air artificially, produce comparatively but an extremely small amount of ozone. If, therefore, a disengagement of electricity took place during the contact of phosphorus with oxygen or atmospheric air, and if this electricity was the cause of the formation of the ozone which occurs under these circumstances, we might expect to observe the most violent electrical phænomena, in a flask where large quantities of ozone were produced under the influence of phosphorus. But we are not acquainted with anything of the kind ; the production of ozone goes on quietly and noiselessly, and no signs of electrical disturbance can in any way be detected. Consequently, if ozone can be formed from common oxygen without the aid of electricity, it appears to me that the term " *Oxygène électrisé* " is altogether inappropriate, and it might with equal justice be called " *Oxygène phosphorisé.*"

2. It is well known that oxygen possesses in many of its combinations the eminently oxidizing properties of ozone, for which reason it appeared to me desirable to express the particular condition of the oxygen in the nomenclature of these substances. This would, however, be difficult if the name " electrified oxygen " is adopted. If, for example, the peroxide of lead is called ozonized oxide of lead, the peroxide of nitrogen ozonized nitrous acid, these names are convenient, and are in harmony with the formula $PbO + O$, $NO^2 + 2O$, which I have proposed for these bodies.

Since the above-mentioned physicists themselves affirm that ozone

* *Comptes Rendus*, Mar. 15, 1852, p. 399 ; Phil. Mag. July 1852, p. 543.

is merely allotropic oxygen, there cannot be any danger of errone-
ous impressions being formed as to the nature of the body, from the
use of the name hitherto employed, to which I shall therefore
adhere until a better one than that proposed by these gentlemen is
found.   Although the experiments of MM. Becquerel and Fremy
have not taught us anything essentially new, still some of their
statements have a peculiar interest ; for instance, the circumstance
that ozone is produced even in a closed glass tube, filled with
oxygen, when electrical discharges are allowed to strike upon its
exterior.   The production of ozone is here evidently the result of an
electrical induction in the oxygen from the exterior and through the
glass.   A similar induction takes place on a large scale on the
occasion of every flash of lightning, a very striking instance of
which I had once an opportunity of observing.   Some years since a
small chapel on the Rhine-bridge at Basle was struck by lightning.
All the rooms in my house, which is about a hundred paces distant,
were filled with a strong odour of ozone at the moment of discharge,
and the same was the case in all the neighbouring houses, so that
the inhabitants of each imagined that their own dwelling had been
struck by the lightning.   It is also deserving of especial notice, that
the smell of ozone was perceived in rooms which were closed, as
well as in those which were in connexion with the exterior atmo-
sphere.   This appeared to me to prove satisfactorily that the ozone
was not carried into these houses by currents of air from the place of
the discharge, but was actually produced in them by induction, and
I have no reason now to consider that this view was incorrect ; in-
deed it is precisely the same fact upon a large scale which the
French physicists have observed on a small one.

M. de la Rive, in speaking of the investigations of MM. Bec-
querel and Fremy, puts forward a new hypothesis for the explana-
tion of the alteration effected in oxygen by means of electricity, &c.
He is of opinion that in ordinary oxygen the atoms are not separate,
but combined in groups forming molecules.   Since, in the chemical
combination of bodies, the atoms unite in single pairs, the cohesion
of the atoms forming a molecule of oxygen would oppose their che-
mical combination with the atoms of other substances, and thus
account for the chemical inactivity which oxygen manifests under
ordinary circumstances towards other bodies.   He regards phospho-
rus, electricity, &c. as possessing the power of breaking up the
molecules of oxygen into separate atoms, on account of which its
chemical activity is increased, and it is rendered capable of oxidizing
bodies at the ordinary temperature.

According to this view, ozone must be considered as atomic and
oxygen as molecular oxygen.   However comprehensible this hypo-
thesis may be, I cannot avoid some hesitation in giving my assent
to it.

1. We must, if we adopt it, regard ordinary oxygen as a body
which is at the same time both solid and fluid. The molecules must
be regarded as solid, inasmuch as they are supposed to be formed

by the strong cohesion of individual atoms. But as ordinary oxygen is gaseous, the hypothesis in question must also assume that each separate molecule acts repulsively upon similar molecules. It might therefore reasonably be asked, why do the oxygen molecules repel each other, while the atoms constituting such molecules mutually attract? When 10, 100, 1000 atoms of oxygen unite together forming one molecule, why does not each such number of atoms combine to form a larger solid body? Why is the oxygen gaseous?

2. Ozone remains unaltered in the cold; by heat it is converted into ordinary oxygen, in which condition it remains after cooling. M. de la Rive must therefore explain this change by assuming that oxygen, consisting of separate atoms (ozone), again assumes a molecular state when its temperature is raised; in fact, that heat facilitates the cohesion of the oxygen atoms, an action the opposite of that which is generally ascribed to this agent.

3. Ozone possesses smell, while ordinary oxygen does not; the former is a violent poison, the latter an indispensably necessary supporter of animal existence. That these great differences in the physiological action of oxygen and ozone should be owing merely to a different state of mechanical aggregation of the elementary atoms, appears to me very difficult to imagine.

4. It is known that by chemical union with certain bodies oxygen acquires the same oxidizing properties as it acquires when free under the influence of electricity or contact with phosphorus. For example, when one equivalent of nitric oxide (NO^2) combines with two equivalents of oxygen gas, the latter enter into a condition of chemical activity precisely similar to that which ozone possesses. It would be difficult to explain how passive oxygen had in this case been converted into action, according to the hypothesis of M. de la Rive. Probably we must assume that NO^2 breaks up the molecules of ordinary oxygen gas, entering into combination with it, and converts it into the ozonized or atomic condition.

Some years since, Mr. Hunt put forward an hypothesis as to the nature of ozone, which is precisely the opposite to that of De la Rive; according to it the ordinary oxygen was in an atomic, and ozone in a molecular condition. Hunt brought forward no facts of any kind in support of his hypothesis, and I remarked at the time that the opposite view might be entertained with equal justice, and I still consider both hypotheses of equal value. So long as we are unacquainted with ozone in a pure state, and especially do not know positively anything of its state of aggregation, specific gravity, &c., it appears advisable to postpone all theorizing on the subject, and especially the advancing of hypotheses which are themselves based only upon hypotheses, such, for example, as that which assumes the existence of atoms. With regard to my own opinions, I do not venture to hazard the most remote conjecture as to the cause of that difference in the properties of ordinary oxygen and ozone, differences which are quite as mysterious as remarkable. I will, however, state that it has never yet entered into my mind to seek this

cause in the state of mechanical aggregation of oxygen atoms, a course which is certainly not very probable in my case, as I entertain doubts as to the correctness of the dogmas of our modern atomic doctrines.—*Journ. für prakt. Chem.* 1852.

ON THE QUANTITATIVE DETERMINATION OF OZONE.
BY C. F. SCHÖNBEIN.

Since ozone combines even in the cold with silver forming peroxide, while ordinary oxygen behaves indifferently towards this metal, I have endeavoured to determine the quantity of ozone in a given volume of air by means of the peroxide of silver formed.

If, for example, 60 litres of artificially ozonized air afforded 100 milligrms. of peroxide, I assumed that it contained 13 milligrms. of ozone, presupposing that ozone was nothing more than allotropic oxygen.

This method, besides its tediousness, is otherwise objectionable, and I endeavoured to discover a more convenient process, in which I believe I have succeeded. Instead of silver I employ a solution of indigo in sulphuric acid; and numerous experiments have convinced me that this reagent admits of accuracy and rapid operation, for the quantity of ozone in several litres of air may be determined by it within a few minutes, even to a small fraction of a milligramme. This method depends upon the property possessed by ozone of decolorizing the indigo solution, a property which ordinary oxygen is altogether destitute of; and, likewise, upon the fact that the most minute quantity of this solution colours a large volume of water. The strength of the indigo solution, which I find the most convenient, is when 10 grammes of it are decolorized by 1 milligrm. of oxygen.

In preparing this test solution, I take 100 grms. solution of indigo prepared according to Berzelius's directions, add an equal quantity of hydrochloric acid, and heat the whole until it boils. I then add to the hot liquid small portions of a dilute solution of chlorate of potash of known strength (one per cent.), shaking the mixture continually until it has become brownish yellow. If, for example, 100 milligrms. of chlorate have been employed to decolorize the indigo solution, I infer that this effect has been caused by the 39 milligrms. of oxygen contained in that quantity of the salt, and consequently that 1 milligrm. of oxygen is capable of decolorizing 100·39 grms. of the solution of indigo. To render this solution of such a strength that exactly 10 grms. of it are decolorized by 1 milligrm. of oxygen, I mix 100 parts with 290 parts of water and preserve it in stoppered bottles.

In order to determine the quantity of ozone in a flask of air containing for example 30 litres, and acted upon to the greatest possible degree by phosphorus, I pour 300 grms. of the test-solution into a glass, and add about one-half to the gas at once. The closed flask is then shaken for some minutes, and a small quantity of the liquid poured out to see if it is decolorized. If so, I dip a small

strip of moist iodide of potassium paper into the vessel, and if this is coloured, add more solution of indigo until the decolorization is complete, when the quantity of solution employed gives the amount of ozone in the gas.

When, for example, 250 grms. of the test-solution are decolorized, the weight of ozone causing this effect would be $250 \div 10 = 25$ milligrms., in which amount there is no allowance for the quantity of air displaced by the 250 grms. of solution. If the volume of the tested gas reduced to $32°$ and 76 centim. bar. amounts to 30 litres, and the weight of ozone in it to 30 milligrms., this air contains $\frac{1}{1298}$ ozone, since under these conditions a litre of air weighs 1298 milligrms, and in this quantity of air there is 1 milligrm. ozone.

My recent experiments have proved that atmospheric air may be ozonized to the extent of $\frac{1}{1300}$ by means of phosphorus; and did not ozone act so energetically upon phosphorus, a much higher degree of ozonization might be attained. At this point, however, the production and consumption of this substance appear to be equal, and ignition of the phosphorus takes place in consequence of the rapid oxidation.

I have already often pointed out the great similarity between the effects produced by chlorine and ozone. One instance of this is the fact that like chlorine it combines with phosphorus at ordinary temperatures. There can therefore be no doubt that this body would immediately take fire in pure ozone gas, as in chlorine, even in the cold.

As the above-mentioned test-solution of indigo is very dark blue, it may be very greatly diluted, and still appear deeply coloured. I therefore employ two more dilute solutions of such a strength, that 10 grms. of one is decolorized by 1·10 milligrm., and 10 grms. of the other by 1·100 milligrm. of oxygen. By this means it is evident that even very small fractions of a milligrm. of ozone may be detected and estimated.

With this very delicate reagent I have found that ozone diluted with 500,000 times its volume of atmospheric air may still be recognised by its smell, sufficiently proving that the pure ozone must have a most intense odour.—*Ibid.*

ON THE MOTION OF FLUIDS FROM THE POSITIVE TO THE NEGATIVE POLE OF THE CLOSED GALVANIC CIRCUIT. BY M. WIEDEMANN.

The author has communicated to the Prussian Academy of Sciences a memoir on the mechanical action of the voltaic circuit, which is of essential interest and importance. The apparatus employed consisted of a porous earthenware cell, closed at the bottom and terminated above by a glass bell firmly cemented to the upper edge of the cylinder. Into the tubulure of the bell a vertical glass tube was fitted, from which a horizontal tube proceeded so as to permit the fluid raised to flow over into an appropriately placed vessel. A wire serving as the negative pole of a battery passed down through the

glass bell into the interior of the porous cylinder, where it terminated in a plate of platinum or copper. Outside the porous cylinder another plate of platinum was placed, and connected with the positive pole of the battery. The whole stood in a large glass vessel, which, as well as the interior porous cylinder, was filled with water. The intensity of the current was measured by a galvanometer. As soon as the circuit was closed, the liquid rose in the porous cylinder, and flowed out from the horizontal tube into a weighed vessel. The results obtained by means of this apparatus were as follows :—

1. The quantity of fluid which flows out in equal times is directly proportional to the intensity of the current.

2. Under otherwise equal conditions, the quantities of fluid flowing out are independent of the magnitude of the conducting porous surface.

To avoid any uncertainty arising from the laws of the flow of liquids through small orifices, Wiedemann measured the intensity of the mechanical action of the current by determining the height of a column of mercury which would hold the transferring force in equilibrium. For this purpose a graduated tube or manometer filled with mercury was attached to the extremity of the horizontal tube above mentioned. With different currents and porous surfaces of different extent, the mercury in the manometer rose to different heights. By the measurements of these heights, the following results were obtained :—

3. The height to which a galvanic current causes a fluid to rise is directly proportional to the intensity of the current, and inversely proportional to the extent of the free porous surface.

The mechanical action of a galvanic current may also be referred to its simplest principles by the following proposition :—

4. The force with which an electric tension, present upon both sides of a section of any given fluid, urges the fluid from the positive to the negative side, is equivalent to a hydrostatic pressure which is directly proportional to that tension.

In this manner therefore we obtain a simple measure of electric tension and its mechanical action in terms of atmospheric pressure, and consequently of gravity.

The above laws hold good only for fluids of the same nature. When different fluids are subjected to the action of the currents, the mechanical action is greatest upon those which oppose the greatest resistance to its passage. The requisite data are still wanting to determine the precise connexion between the mechanical action and the resistance ; but observations made with solutions of sulphate of copper of different degrees of concentration, appear to show that the quantities of fluid transferred in equal times by currents of equal intensity are nearly proportional to the squares of the resistances.— Silliman's *Journal* for November 1852, p. 420.

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END OF THE FOURTH VOLUME.

(Bowman)

(Barry)

J. De C. Sowerby. sc.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

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